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A PROJECT FOR THE PERMANENT IMPROVEMENT OF THE CHANNEL OF ENTRANCE INTO THE HARBOR OF CHARLESTON, S. C., BY MEANS OF LOW JETTIES.

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THE CHARLESTON BAR.

The bar which stretches bow-shaped across the entrance into Charleston Harbor, from Sullivan's Island on the north to Folly Island on the south side, has not varied much in either location, general direction, or magnitude, within the period covered by any trustworthy knowledge which we possess on the subject.

A comparison of the chart of 1780, published in Des Barrés' Atlantic Neptune, with those of 1821, 1825, and 1851–'52, "shows that according to the earliest records the bar of Charleston has varied comparatively but little in extent, direction, or in distance, from the mouth of the harbor."

Measured along its crest, or line of least depths, the bar is ten miles in length, its north end on Sullivan's Island being close up to the entrance or throat of the harbor, while its south end, resting on Folly Island, is six miles distant therefrom. Its average width between the 18-foot curves is about $1\frac{3}{4}$ miles.

In many places the highest points of the bar are only three to four feet below the level of mean low-water, although the average depths along the crest are considerably greater.

The main central body of the bar, lying nearly due north and south, is almost straight for a length of over five miles, has its crest parallel to the main shore, south of the entrance and at a mean distance of about two miles from it, and is not at the present time, and, so far as we know, never has been traversed by practicable ship-channels.

The northern and southern extremities of the bar are formed by rather sharp curves, which connect the straight portion already mentioned with the shore above and below the harbor.

So far as we can now ascertain there appears never to have been less than four, nor more than six, ship channels across the bar at any one time. The greatest depth of water has sometimes been found in one channel and sometimes in another, being rarely less than $11\frac{1}{2}$ feet, or more than $13\frac{1}{2}$ feet, at mean low tide.

The channels, whether four or more, have always existed in two groups or clusters, one in the northern and the other in the southern curved portion of the bar, and there has always been a

deep and broad anchorage inside the straight reach of the bar abreast of Morris Island.

This anchorage, sometimes called the "main channel" and sometimes the "outer harbor" varies in width from one-third to two-thirds of a mile between the 18-foot curves, and in maximum low-water depths from 20 to 45 feet. The direction of its central line is about north and south, and its length from the throat of the harbor between Morris and Sullivan's Islands to its southern terminus, where it spreads out in various channels and shoals in crossing the bar, is fully five miles. At the extremities of this outer harbor or basin, several miles apart, are found the two groups of channels already mentioned, the most northerly group being directly in front of the gorge of the harbor.

The bar is essentially a drift-and-wave bar, produced in part by the upheaving action of the waves when they approach the shore, and are converted by breaking into waves of translation, and in part by drift-material carried along the coast by surf-currents, especially by those produced by northeast storms. The peculiar location of the bar, largely to the southward of the gorge of the harbor, and the conditions under which a very large proportion of the ebb-flow is diverted from its most direct path, and forced to skirt the main coast for several miles before it can find a passage to the sea, indicate the controlling power of these storms.

The material composing the surface of the bar closely resembles that usually found on the sea-shore between high and low water in that section of the country, being shells and fragments of shells, or silicious sand, or a mixture of them all. It is easily thrown into suspension by waves, and is moved by a moderate current.

On the north end of the bar five borings were made in order to determine the character of the substrata. The points selected for boring, and the results obtained, are indicated on the accompanying drawings.

It will be seen that below the surface there are some layers or lumps of mud, as well as of mud mixed with sand, and mud mixed with shells.

All the channels which traverse the bar are, and, so far sa we know, always have been, ebb-tide channels, produced and maintained mainly by the scour of the ebb-current, except Beach (formerly Maffitt's) Channel, the most northerly of them all, which lies close to Sullivan's Island. This is a flood-tide channel, possessing the usual characteristic of such channels, that their least depths are always found near their inner ends, and therefore in comparatively quiet water. Another distinguishing feature of such channels is that from the cross-section of shoalest soundings inward, toward the harbor, the descent into deep water is sharp and sudden, while outward, toward the ocean, it is gradual and gentle.

The North or Cumberland Channel at the entrance into Cumberland Sound, Georgia, and the Coney Island Channel of New York Harbor are of the same character. In speaking of the preponderance of the flood over the ebb in Cumberland Channel, in my report on the jetty system as applied to the entrance into Cumberland Sound, Georgia, submitted April 15, 1876, I say:

"The effect is to make the inner slope of this part of the bar very steep; the sand which is rolled along by the flood-current on the bottom of the outer slope is first brought to rest in the deep water of the inner basin. The ensuing ebb-current, which receives its velocity and direction from the large volume of Cumberland Sound, sweeps the inner slope of the northern shoals longitudinally, and takes up this sand and carries it out by the Amelia Basin, depositing it upon the main bar. The channel next to Cumberland Island is therefore a flood-tide channel, like the Sullivan's Island or Beach Channel in Charleston Harbor. They both possess in a marked degree the steep inner slope which invariably characterizes a channel maintained by the flood-tide, which having once passed in is so much diverted in its direction on the ebb, by the axial line of the tidal basin, that it cannot flow out in full volume through the same opening, but sweeps past its mouth in its passage to some more direct outlet."

Beach Channel was gauged during the months of May and June, 1876, when it was found that on a section taken 500 yards east of the inner end of the channel, at the Bowman jetty, the

volume flowing out during an entire ebb between the low water line on Sullivan's Island and the 5-foot curve on Drunken Dick Shoal, amounted to only 48.8 per cent. of the volume flowing in during an entire flood through the same section.

On a section taken 930 yards east of the Bowman jetty, between the low-water line on Sullivan's Island and the 10-foot curve on Drunken Dick, the volume of ebb amounted to 52½ per cent. of the flood.

CAPACITY OF THE TIDAL BASIN.

The area of the tidal basin formed by Charleston Harbor, as computed from the Coast Survey chart and Mills' Atlas of South Carolina, is about 15 square miles. This area is assumed to be filled during each mean flood-tide by a layer or prism of water 5.1 feet in height above the mean low-water level. In addition to this the adjacent reaches of the tributary rivers will be filled above their low-water stage by flood and back waters, which at the period of slack water-flood will form in each stream a wedge-shaped mass resting on the sloping low-water line of the river, and extending up to a point where the influence of the tidal wave ceases to produce a rise and fall of the surface of the water. The equivalent of these wedge-shaped masses, determined by simultaneous tide levels, together with the water derived from land drainage during the ebb flow, will be added to the volume of the tidal prism above mentioned.

In other words, the total volume of outflow during each ebb tide, will be measured by the volume contained between certain planes of low water and of high water, throughout the area of the tidal basin, and up the streams to points where the tide ceases to be felt, augmented by the volume derived from land drainage during the period of ebb flow.

In order to make a reasonably close estimate of the volume of outflow, it would be necessary to determine the plane of low and of high water, by a series of simultaneous water levels taken in the tidal basin and its branches, supplemented by a survey sufficiently in detail to give the high water and low water areas of the basin and branches, and an accurate topography of the marginal low lands situated between high and low water.

No investigations of this character having been made at the harbor of Charleston, the information derived from the sources above named will be mainly relied upon in this discussion.

From these data it is estimated that the average discharge through the throat of the harbor between Sullivan's and Morris Islands, on each ebb during the period of mean rise and fall of tides, amounts to a little over 3,655,443,686 cubic feet. Of this volume only about 76,571,000 cubic feet is supplied by the land drainage, on the assumption that one-half the rain-fall reaches the sea. This estimate is believed to be not too large, in view of the fact that the streams are short and in close proximity to the points of discharge.

For two or three days during the period of spring-tides, the average ebb-discharge will be augmented to about 4,228,846,000 cubic feet. The neap discharges, being in smaller volumes than those pertaining to mean tides, require no special mention, as any temporary decrease of scouring power in the new channel beyond the jetties resulting therefrom would be of short duration. Even if slight shoaling ensued during this period, the maximum depths established by mean and spring tides would be restored on the return of these tides.

The mean duration of the ebb-flow is taken at six hours, that being the average of a number of observations made by Civil Assistant George Daubeney, in 1870 and 1871, the longest flow being 6h 20m, and the shortest 5h 25m.

The average ebb-discharge per second through the gorge of the harbor during the period of mean rise and fall of tides is therefore 169,233 cubic feet ($\frac{3655443686}{21600}$), and during the period of spring-tides 195,780 cubic feet ($\frac{4228846000}{21600}$), the average rise and fall at ordinary spring-tides being 5.9 feet. No account is here taken of the somewhat longer duration of ebb-flow at average spring-tides.

During very high spring-tides the discharge will be much larger. With a rise and fall of 10.3 feet (which has actually occurred), the prism amounts to about 7,382,562,000 cubic feet, equivalent to 341,780 cubic feet per second;

nor will this show the total discharge, since the marshes will be flooded, and their area being estimated at eight square miles, every layer of water over them three inches thick will add 55,965,870 cubic feet to the prism, or 2,590 cubic feet to the average discharge per second.

It has not been deemed expedient, or likely to give trustworthy results, to attempt to gauge the flow through the gorge of the harbor by means of current-velocities. Those taken some years ago between Forts Sumter and Moultrie, with a view of locating channel torpedoes, proved the existence of eddies and counter-currents, and other irregularities of flow, to such degree, especially near the Sullivan's Island side, that the requisite accuracy seemed hardly obtainable by this method.

There is nothing specially exceptional in this, for it is known that abnormal conditions often characterize the flow of water through the gorge of a large tidal basin.

It is stated by Mr. D. Stevenson that at Cromarty Firth, where the waters pass to and from the sea through a narrow gorge, of which the width is about 4,500 feet and the depth about 150 feet:

> The mean velocity due to the column of water passing this gorge, as deduced from the observed surface-velocity, was not sufficient to account for the quantity of water actually passed during each tide, as determined by measuring the cubical capacity of the basin of the Firth. This led to the observation of the under-currents through the gorge by means of submerged floats, and it was found that during flood-tides the surface-velocity was 1.8 miles per hour, while at the depth of 50 feet the velocity was not less than 4 miles per hour, being an increase of 2.3 miles per hour. During the ebb-tide the surface-velocity was 2.7 miles per hour, and at 50 feet depth it was not less than 4.5 miles per hour, being an increase of 1.8 miles per hour.

Anomalous variations and irregularities between the surface and the sub-current have also been found to exist in the harbor of San Francisco, Cal., and elsewhere.

For the foregoing reasons, mainly, it has been thought best to use the cubical capacity of the tidal basin and the rainfall upon the drainage-area in estimating the average volume of water which flows out and in through the gorge of Charleston Harbor.

PLAN OF IMPROVEMENT RECOMMENDED.

It is proposed to construct two low jetties, one springing from Morris Island and the other from Sullivan's Island, converging toward each other in such manner that their outer ends on the crest of the bar shall be one-half to five-eighths of a mile apart. The outer ends of the two jetties will rest respectively upon the shoals lying to the northward and southward of what is known as the north channel, that being the middle channel of the north group of three channels, and having its line of deepest water located more nearly than either of the others upon the prolongation of the axis of deep-water flow through the gorge of the harbor between Cumming's Point and Fort Moultrie.

Assuming for the purposes of discussion the sea ends of the jetties to rest respectively at X and Y, it seems, in some measure, immaterial whether they be established upon straight lines as shown at AX and BY, Plate I, or upon curved lines; and if curved, whether the convexity be turned toward the central channel as at CX and DY, or from it, as at EX and FY. In either case, if kept at the proper heights, they will produce an ebb-flow through the gap able to maintain a deep channel through the bar. Neither the straight jetties, however, nor more especially those with their convexity turned away from the channel, act as training-walls to guide the outflowing water. The curved jetties convex toward each other, being less open to this objection, are the ones adopted in this project.

The north jetty starts from a point on Sullivan's Island 1,800 yards east of Bowman's jetty. The half next the shore is curved to a radius of about 1½ miles, the outer half being very nearly a straight line. The total length of this jetty from C to X is 7,450 feet, and its general direction is southeast.

The south jetty, having a total length of 11,650 feet from D to Y, starts from Morris Island at a point about 650 yards from Cumming's Point, its general direction being east. The shore end is curved to a radius of about three miles for a little more than one-half its entire length, while the half next the sea is nearly straight, as in the case of the north jetty.

The specified length of the jetties is

taken for purposes of discussion. As will be seen hereafter, they would not be able to produce a channel of the requisite capacity through certain materials which are likely to be encountered in the bar, although they would be expected to maintain such a channel if once established.

The outer ends of the two jetties slightly converge toward each other as they approach the crest of the bar, and are intended to act as training-walls for a distance, in each case, quite equal to half its entire length. These portions lie in the direction of the flood-currents, and may be built to any height without obstructing the inflow. For fully one-fourth of their entire length the sea ends could be carried above the level of high-water, so as to be visible at all stages of the tide.

The characteristic feature of the design—that of low jetties—is intended to maintain the bar in its present general location, with such moderate increase of magnitude as may be expected to result from concentrating upon a gap one-half to five-eighths of a mile in width, a portion of the water which is now dispersed over a width of ten miles.

The complete success of the works is believed to depend on three important conditions, which they are expected in great measure to satisfy, and which have been kept in view in preparing the design, viz:

1. *They should not impede the inflow to such degree as to prevent the tidal basin being filled as now at every influx of the tidal wave.*

To this end the inner half of each jetty, more especially its central portion, located in deep water across the thread of the current, is kept several feet below the water. The outer half, being nearly parallel to the direction of the flow, is built higher, and the sea end, for a distance of several hundred feet, may be carried up to high water level, or higher.

2. *They should control the outflow to such degree and in such manner that a channel of the required depth will be maintained through the bar.*

To this end, although a large portion of the *surface flow* will spread out over the tops of the jetties and thence over the bar, the *central flow*, throughout the entire depth along the axial line of the gorge between Sullivan's and Morris Islands, is aided in its natural tendency to reach the sea along the prolongation of that line, by the opening left for it between the jetties. The *bottom-flow* through the gorge of the harbor is deflected on converging lines by the jetties, and is therefore forced in a measure to concentrate itself in, and flow out through the gap between them. The outer half of each jetty and the adjacent portion of the shore end act as a training-wall for this flow.

3. *They should not to any considerable extent cause a movement seaward of the main body of the bar; that is, the general position of the bar should be independent of the effects produced between and beyond the heads of the jetties.*

It is believed that this condition will be secured by making the shore ends of the jetties low for at least one-half their length, or throughout those portions which cross the thread of the current in deep water, so as to allow the tide to ebb and flow somewhat freely over them. The effect of high jetties, with a correspondingly wide gap between them to allow a full influx of the tide, would tend to transfer the gorge of the harbor from its present position to the sea ends of the jetties, two and a half miles distant, and move the shore line out to that point, by causing a filling in of the exterior angles between the jetties and the shore. After reaching this stage, a drift-and-wave bar would probably be found to the seaward of the present bar, in front of the jetties, rendering it necessary to extend them in order to cut a passage through it.

It seems essential, therefore, that the agencies which maintain the present bar should remain in as full force as possible, consistent with the requisite concentration of outflow between the jetties.

The probable effects will be that the bar will be raised somewhat throughout its entire length, the waves will break upon it more frequently than now, and considerable shoaling will, of course, take place in Beach Channel and in all the southern group of channels. But it is believed that the important condition of keeping the bar generally in its present position will be secured.

The drift-material carried along the coast by surf-currents, as well as the sand thrown up by the breakers on the

north and south shoals, instead of lodging in and filling up the exterior angles between the jetties and the shore, as in the case of high jetties, will be disposed of in a harmless manner.

For example, a heavy northeasterly storm, producing breakers along the north shoal, and strong southerly surf-currents along the shores of Long and Sullivan's islands, would put in motion a large quantity of material, a portion of which would be carried in by the flood-currents over the north jetty and through Beach Channel, coming to rest in the deep water of the main channel. It would next be taken up by the ebb current and rolled out to sea between the jetties. Beyond the jetty-heads it would encounter the littoral ebb-current, moving to the southward with a velocity accelerated by the storm, by which it would be again carried in a southwesterly direction until finally, left to the action of the south breakers, it would be either deposited temporarily upon the south shoal, or carried still farther to the southward. This action, which would be incessant during the continuance of the storm, is illustrated in Figure 1, Plate III.

The action of a southerly storm would be the reverse of this. In either case some drift-material would be carried by waves and surf-currents around the jetty-heads, and would subside in the deep water between them, to be swept out by ensuing ebb-currents, and disposed of to the northward or southward, according to the direction of the storm.

This movement of sand was referred to in my report on the improvement of the Fernandina Bar, submitted April 15, 1876, from which the following extract is made:

> As a moderate assumption, a northeaster of three days' duration might be expected to lower the north shoal four inches within the area covered by the breakers. The greater part of the eroded material, amounting to upward of 516,000 cubic yards, would doubtless be distributed along the south shoal during the progress of the storm. If the waves should subside, or a southerly or southeasterly storm set in before the bar channel had returned to its nominal condition, the material subsequently carried out would not reach the south shoal, but in the former case would remain near the outlet on the outer slope of the bar, and in the latter would be carried back by the waves to the north shoal. If as much as one-fourth of it remained in the bar channel between the inner and outer eighteen foot curves, a few severe storms such as frequently occur within the period of a single month would entirely destroy it, by filling it up to the level of the shoal on either side.
>
> It would appear, therefore, that millions of cubic yards of the material composing the bar might be shifted back and forth from one side of the channel outlet to the other during a single season, without causing injury to the channel by shoaling, and without producing any changes in the form and location of the bar itself, that might not entirely escape the notice of the most careful surveyor. And yet this shifting of material of which no evidence may be left behind, should enter as an important, if not a controlling function in the project of the engineer, because the useful life of his works is more or less dependent thereon.

As no works can be expected to stop this movement of drift-material for any great length of time, they should, if practicable, accommodate themselves to it under conditions of a permanent character. Those proposed are designed to do this, by allowing the drift-sand to move from one part of the bar to the other in much the same manner as now, never remaining in the jetty channel longer than a few tides, and never finding a resting-place anywhere that the next storm may not disturb.

PROBABLE EFFECT PRODUCED BY THE JETTIES.

An attempt is made below to determine by the use of appropriate formulæ the principal phenomena of the ebb-flow, after the jetties shall have been constructed and an enlarged water-way of the greatest self-maintaining area has been established between them, and the hydraulic equilibrium has been restored.

The jetties in this discussion are first assumed to occupy the lines CX and DY, Plate I, with their respective crests established at the varying heights shown by the longitudinal sections CX and DY on Plate II, the sea ends being half a mile apart. The north jetty crosses the deep water of Beach Channel at the level of twelve feet below mean low-water, the crest being held at that level for a length of about 650 feet, whence it rises gradually by gentle slopes to high-water at each end. On the sea end the part carried to high-water level is 1,500 feet long.

The south jetty, designed on a similar plan, crosses the main channel on a level

fifteen feet below mean low-water, the seaward end for a length of 2,000 feet having its crest at high-water.

The sectional area of the gorge profile between Morris Island and Sullivan's Island is as follows:

	Square feet.
Area of low-water section....	159,550
Area of high-water section...	195,350
Mean ebb-tide area.....	176,600

The width of the surface at half tide, corresponding to the mean ebb-tide area is 6,825 feet, and the wetted perimeter 6,927 feet. The hydraulic radius is, therefore, 25.46 feet.

At mean low-water the surface width is 6,750 feet, the wetted perimeter 6,851 feet, and the hydraulic radius 22.29 feet.

The area inclosed between the line of gorge at Cumming's Point (Morris Island) and that of the proposed jetties and gap is 2.16 square miles.

The average discharge per second across the proposed sites of the jetties and the gap between them is, therefore, 183,451 cubic feet

$$\left(\frac{3,655,443,886+307,168,434}{21,600}\right)$$

or 14,218 cubic feet more than the amount flowing out at the gorge.

The following are the sectional areas in square feet now existing on the lines proposed for the jetties and gap:

	Low water.	Mean half tide.
	Square feet.	*Square feet.*
Line of north jetty.	59,900	78,880
Line of south jetty.	171,720	201,365
Gap.....	22,840	29,572
Totals.....	254,460	309,817

For the following calculations the D'Aubuisson-Downing formula will be used, not because it is the best, but mainly because it is very simple and easy of application. It is, moreover, believed to answer very well in cases of broad open streams.

The formula is

$V=100\times\sqrt{s}\times\sqrt{r}$, in which

V=velocity in feet per second.

s=slope, or ratio of horizontal length to vertical descent.

r=hydraulic radius in feet.

In the gorge at Cumming's Point the grand mean of all the velocities is .958 feet per second $\left(\frac{169,233}{176,600}\right)$.

The grand mean of all the velocities with which the water passes through the various compartments of the present section along the line of the jetties and gap is 0.59212 feet per second $\left(\frac{183,451}{309,817}\right)$.

The mean hydraulic radius of this aggregate section is 14.0322 feet

$$\left(\frac{309,817}{7,574.3+2,679.7+11,825}\right).$$

Therefore

$$V=0.59212=100\sqrt{14.0322}\times\sqrt{s}.$$

$$\sqrt{s}=\frac{0.59212}{100\times\sqrt{14.0322}}=0.0015807$$

$$s=0.000002498.$$

On the assumption that this slope is the same throughout the section (which in point of fact is not precisely the case, and we have no data for making the necessary correction for the several compartments), the total average volume of discharge per second, amounting to 183,451 cubic feet, is distributed as follows, as determined by the various areas and hydraulic radii:

	Cubic feet.
Through present section on site of north jetty.....	39,418
Through present section on gap.....	15,227
Through present section on site of south jetty.....	128,806
Total as above.....	183,451

This will be assumed to represent the present distribution of the outflow per second through the section selected for the sites of the works and the opening between them at its narrowest point.

The changes of regimen which the jetties will tend to produce, and the area of the water-way which once established they would be expected to maintain between and beyond the sea ends, will next be considered.

The north jetty will reduce the half-tide area of the water-way from its present area of 78,880 square feet to 41,593 square feet, and the hydraulic radius from 10.41 feet to 7.59 feet.

The south jetty half-tide water-way

will be reduced from the present area of 201,365 square feet to an area of 94,684 square feet, and its hydraulic radius from 17.03 feet to 10.77 feet.

These hydraulic radii are to be considered permanent, the crests of the jetties being supposed to be able to resist abrasion by the current.

In the gap, where alone erosion can take place, the present mean half-tide water-way is 29,572 square feet, and the mean low-tide area 22,840 square feet.

After the jetties shall have achieved their maximum scour, aided by dredging or other artificial appliances wherever clay-beds are encountered, and the equilibrium of flow is resumed, the original general average slope S=0.000002498 will be restored.

The aggregate average discharge per second before the jetties were built will also be restored.

From these premises the following average discharges per second are found:

	Cubic feet.
Across crest of north jetty.....	18,113
Across crest of south jetty.....	49,110
Total over the jetties.....	67,223

The balance of the discharge, amounting to 116,228 cubic feet per second (183,451—67,223), will go out through the gap between the jetties, where at present there is a mean half-tide area of only 29,572 square feet and a mean discharge of 15,227 cubic feet per second.

The formula already used gives for the average velocity through the gap:

$$V=100\times\sqrt{s}\times\sqrt{r}$$

Substituting the value $\sqrt{s}=0.0015807$, we have

$$V=0.15807\times\sqrt{r}$$

The value of r is unknown. The width of the gap being 2,640 feet, we have for the wetted perimeter, by General Abbot's rule, 2,680 feet (2,640×1.015).

If A represent the unknown half-tide area of the gap in square feet, we have

$$r=\frac{A}{2,680}$$

and

$$v=0.15807\times\frac{\sqrt{A}}{\sqrt{2,680}}$$

The calculated average discharge through the gap per second being 116,228 cubic feet, we have

$$116,228=Av=A\times\sqrt{A}\times\frac{0.15807}{\sqrt{2,680}}$$

$$A=\sqrt[3]{\left(\frac{116,228\times\sqrt{2,680}}{0.15807}\right)^2}$$

A=113,160 square feet.

The mean hydraulic radius at the gap will therefore be 42.22 feet $\left(\frac{113,160}{2,680}\right)$ at mean half tide, or 39.71 at mean low-water. This implies very considerable mid-channel depths.

In the profile between Fort Sumter and Sullivan's Island, having a mean low-water area of 177,620 square feet, a width of 4,960 feet, and a hydraulic radius of 35.28 feet, fully ninety per cent. of the total area pertains to depths of twenty-four feet and upward, occupying a width of 3,540 feet, in which the maximum depth is seventy-six feet.

On the profile from Cumming's Point to the Bowman jetty, the low-water area is 159,550 square feet, the width 6,750 feet, and the hydraulic radius 23.29 feet. The compartments of twenty-four feet depth or more form eighty per cent. of the whole section, and occupy a width of 3,000 feet, with maximum depths close up to seventy feet.

In the new channel between the jetty-heads, where the hydraulic radius is 39.71 feet, it may be expected that the area of depths of more than twenty-four feet will constitute a very large proportion of the total area of the gap, and that maximum depths of seventy-five feet and upward would be maintained in mid-channel.

The average velocity from which the general average slope is derived is, of course, less than the velocity that will prevail in the deep channel compartments of the profile, since with unaltered slope the velocities in different portions of the profile may be considered to vary as the square root of depths. The grand average velocity in the profile between Cumming's Point and Bowman's jetty, with a mean hydraulic radius at half tide of 25.46 feet, is .958 feet, per second; in the 50-feet compartments the *average* velocity would be 1.33 feet per second; while during the second and third quarters of ebb the velocities will vary between two and three feet per second.

The bottom velocities will generally be but little less, to judge from the results of a great number of current observations made near Fort Sumter by Capt. William Ludlow a few years ago.

Of the effects that will be produced to the seaward of the jetties upon the outward slope of the bar, by so large a volume of outflow, it is impossible to deduce from formulæ, results upon which reliance can be safely placed. We know what kind of effects will ensue, but we have no precise measure of their intensity. The first and greatest difficulty met with is the want of trustworthy data concerning the rate at which the water, as it issues forth from the gap, will spread out and disperse over the descending outer slope of the bar, with a diminishing velocity and scouring power. For the purpose of discussion, it will be assumed that the currents having passed the jetty-heads will spread out in a fan-shaped area, at an angle of thirty degrees on each side, with the axis of the new channel. The chart seems to indicate that this angle is not too small. It is, however, largely conjectural.

Assuming, however, a total spread of sixty degrees, the width of the profile $1\frac{1}{2}$ miles to seaward, through which the outflow from the jetties is supposed to pass, is 10,933 feet.

By adding the fan-shaped water-prism between the jetty-heads and the seaward profile to the volume of flow through the former, we find that the average volume passing through the outer profile will be 128,916 cubic feet per second.

The half-tide sectional area of the profile is found, by the method of calculation already employed, to be 172,312 square feet. Its wetted perimeter is 11,097 feet, its hydraulic radius at mean half-tide 15.52 feet, and at mean low-water about 13 feet, which implies more than ample mid-channel depths through the outer slope of the bar for vessels of the deepest draught.

As this outer profile is taken upon the seaward slope of the bar a little beyond the eighteen foot low-water curve, the permanent depths first secured there—permanent because representing a restored equilibrium—can, of course, be increased at pleasure, and at a small relative cost, by the moderate extension of the jetties.

If the gap between the jetties be widened, the submerged portions must be raised to a greater average height, thus diminishing the area of water-way above them, in order that a channel of the same mean depths in the seaward profile near the outer eighteen foot curve, above deduced for a specified height, may be maintained. Considerations of cost furnish strong arguments for keeping the crest of the jetties low, as the expense of added height in jetties with side slopes increases much more rapidly than the height itself. For example, a wall ten feet high and ten feet wide on top, with slopes of forty-five degrees, contains 200 square feet in cross section, while a wall of the same width on top and only twice the height contains three times that area of cross-section. By doubling the height the quantity of materials required is therefore trebled in this case, and more must be still added to compensate for the increased subsidence caused by doubling the weight on the foundation. By trebling the height we get six times the area of cross-section.

With an opening between the jetties five eighths of a mile wide, established by swinging the south jetty to the southward around its shore-end as a center until it occupies the line DZ, and leaving the north jetty located on the line CX, as before, it will be necessary to raise the submerged portions an average of about 14 inches higher than the crests shown on the longitudinal sections CX and DY, Plate II, in order to maintain in the seaward profile $1\frac{1}{2}$ miles from the jetty-heads, the same hydraulic radius deduced for the half-mile gap. Between the jetty-heads the hydraulic radius for the five-eighths mile gap would be about 4.45 feet less than for the half-mile gap. Under both suppositions the sea-ends of the jetties rise to high-water level for a length of 1,500 feet on the north jetty, and 2,000 feet on the south jetty.

There seems to be little room for doubt that a channel of ample capacity having been once established through the bar, it will be permanently maintained by the jetties, and that the materials more or less constantly carried out by the current,

especially during the prevalence of drift-producing storms, and immediately subsequent thereto, will not be deposited under conditions favorable to the formation of an exterior bar.

The outer slope of the bar, directly to the seaward of the jetties, will perhaps assume and maintain a salient form in consequence of the materials being first brought to a temporary rest at that point; but unless the main body of the bar to the northward and southward of the jetties also moves bodily to the seaward in a marked degree, in violation of all known or suspected laws, the movement of drift will go on substantially as at present, finding only a transient resting place in front of the new channel, or upon any other portion of the bar.

Having assumed the width between the jetties and the points on the bar at which their sea-ends should rest, it is not claimed that the corresponding height capable of maintaining through the bar, to deep water on the outside, a channel of a specified capacity, can be determined with precision by computations based on the use of any known formulæ. But it seems quite clear, with the large surplus of available water not needed between the jetties, that we can by first building them low throughout their entire length, and then raising them gradually to the required height, utilize the flow, and accomplish the desired results, not only with certainty, but with the greatest attainable degree of economy.

It will be expedient, from other considerations, to proceed gradually in raising the works to the requisite height. It will be seen from Plate III, containing a record of the borings, that at the point D, nearly in the axis of the new channel, and a little outside a right line joining the sea-ends of the jetties, a bed of "soft mud and sand," 7 feet in thickness, is encountered at a depth of 5 feet below the bottom, and 17 feet below mean low water. It overlies a bed of sand 4½ feet thick. At E about 460 yards to seaward of the point D, and also in the line of the new channel, a layer or thin bed of sand, shells, and soft mud, only 1 foot thick, is found 1 foot below the bottom, and 13 feet below low water. At a depth of 6½ feet, a 1-foot bed, or lumps of stiff clay exist, resting on 10½ feet of fine sand. At A, more than 1¼ miles inside the jetty head, and a little to the northward of the probable line of deepest water, a bed of tenacious clay is found 4 feet below the bottom, and 18 feet below low-water, while outside the gap at F, about half a mile in a southerly direction from D, no clay or mud is found until a depth of 28 feet below low water is reached, and there it is only a foot thick, and rests upon 3 feet of "shells and sand."

These borings show that the material which may be found capable of resisting erosion and removal by the currents does not occur in continuous and regular strata, but apparently in detached sheets, lumps, and beds, varying greatly in thickness and in depth below the bottom, and below the water-level.

It is presumed that none of the materials which it would be necessary to remove, in establishing a deep water-channel through the bar, can be eroded and carried off by the currents, except those designated in the table of borings as "shells," "sand," "soft mud," or a mixture of two or all of them. Whenever stiff clay is to be removed some method of dredging or harrowing will have to be adopted, and it may be necessary to resort to harrowing in aid of the natural scour, to get rid of some of the beds of mud and softer clays. The sand and shells will be carried out by the current.

When the jetties, supposed for the present to be built of riprap resting on a mattress of fascines, have reached their full length, or rather their assumed length, from the shores to the points X and Y, respectively, with heights throughout the submerged portions not much greater than may be deemed necessary to secure the foundations from injury by undermining, the lower sections should then be gradually built up until a sufficient flow is established between them to scour off the surface-layer of sand, shells, and soft mud, and lay bare the beds of stiff mud and clay between the heads of the jetties, and as far beyond them as possible, consistent with the safety of the works themselves. The greatest effect will naturally be produced along the center line, and the volume of flow should not be made large enough to cause any considerable scour along the faces of the jetties.

Dredging, if it becomes necessary at

all, should begin along the line of greatest scour as soon as the removal of the clay by that method becomes practicable, and as greater depths are secured in this manner the jetties should be raised to higher levels.

The borings indicate that sooner or later, during this stage of progress, it will become necessary to determine in what manner the needed depths to seaward upon the outer slope of the bar can best be established. It may be done either by enlarging the area of the water-way between and directly in front of the jetties, so as to lengthen the outward reach of the scouring power, or by extending the jetties themselves further out on the bar, with only moderate depths between them, thus carrying further to seaward the point at which divergence and consequent loss of power begin. In the degree to which the first method, if adopted, is carried into execution, will the jetties approach the heights shown in sections CX and DY, Plate II, and they could not theoretically attain and exceed those heights until the channel in the gap has a mean half-tide area of 113,160 square feet, and a hydraulic radius of 42.22 feet. This implies, as already stated, a deep central channel with maximum depths, which would perhaps be impossible of attainment at moderate cost by any known process of dredging or raking.

The boring at D, in the line of the new channel, indicates that very little dredging or raking would have to be done to reach a depth of 31 feet below mean low-water, there being only 6 inches of stiff clay to penetrate in that distance, and that is found at a depth of 28½ feet. At E, farther out on the same channel line, only 12 inches of stiff clay is encountered in a depth of 30 feet. Whether this material occurs nearer the surface, or in thicker beds, at other points where its removal would be necessary to give the requisite water-way, cannot, of course, be known from the examinations that have been made. Very numerous borings taken near each other would be necessary before even a very general estimate could be made of the quantity of materials of different kinds that would require removal by other agencies than the natural scour, in order to attain any given area of water-way.

It is probable that the thin deposits of clay encountered in boring are only detached lumps or small masses that will be no obstacle to the prosecution of the work, but will settle down to lower levels as the sand is scoured away from around and beneath them. The existence of such lumps on the bottom of the inner harbor has been reported by divers.

For the purpose of this estimate, maximum mid-channel depths in the gap of only 31 feet at mean low-water will be adopted, because that depth appears to involve only a small outlay for dredging, and possibly none at all.

By fixing the crests of the submerged portions of the jetties at the requisite heights, we have the means of maintaining in this water-way average depths not much less than the maximum depths, thus producing a wide channel with moderate depths, instead of a narrow channel very deep along the central line and shoal toward the sides. Under these conditions the hydraulic radius in the gap can be made comparatively large. It will be taken at 24 feet mean half-tide.

It appears from calculations based, as before, on an assumed divergence of 60 degrees in the ebb flow exterior to the jetty-heads, that a normal flow through the half-mile gap, with a hydraulic radius of 24 feet, cannot maintain a channel exceeding 21 feet in depth at mean low-water, for a greater distance than about 5,500 feet beyond the heads of the jetties where the divergence begins. This would require the jetties to be 2,400 feet longer than jetties CX and DY, already discussed, although their submerged crests would be somewhat lower. The north jetty, if kept generally parallel to the bottom, would not exceed 1 foot in average height, its office being mainly to prevent the enlargement of the Beach Channel water-way by scour. The south jetty would have its submerged crest at 10.78 feet below mean low-water, if kept level throughout. Under these circumstances, with a 24-foot hydraulic radius in the gap, and corresponding hydraulic radii in the seaward profiles, on the supposed total divergenee of 60 degrees, the original slope will be restored. The mean average ebb velocity through the gap will be 0.93 foot per second.

By raising their submerged portions above the calculated heights, last mentioned, greater ebb flow and velocities would be established in the gap, with correspondingly increased power and outward reach, and, therefore, increased depths through the outer slope of the bar into the deep water beyond. But this would give no greater depths in the gap, under the supposition that beds of clay exist there at and below the depth of 31 feet, the only condition which appears to impose the necessity of low jetties at all.

If the submerged crests be placed at the varying heights shown in sections CX′ and DY′, Plate II, the total areas over the jetties and through the gap will be somewhat diminished, and as the areas are all fixed, while the volume to be discharged remains the same, there will ensue in the gap a banking up of the waters and consequently an increase of slope and of velocity. The computations show that the natural slope of 0.000002498 or about $\frac{5}{32}$ inch to the mile, will be increased to 0.000004963, equal to about $\frac{5}{16}$ inch per mile; and the previous mean average velocity of 0.93 foot per second will be augmented to 1.09 feet per second. At what distance beyond the jetty-heads the original slope will be resumed cannot be ascertained by any process of computation, and consequently the distances beyond the points X and Y, to which the jetties should be carried in order to maintain a channel of the required depth through the outer slope of the bar, is largely conjectural. It is certain that they will not have to be extended as far as in the case of the low jetties last discussed. The calculations show, however, that, with the assumed divergence of 60 degrees, the heads of the jetties, or the point where divergence begins, need not be located more than 1,390 feet to seaward of the points X and Y, Plate I. This, theoretically, places their heads at X′ and Y′, respectively.

The practical solution of this question would of course be given by a gradual and cautious building up of the jetties, with frequent observations of their effects, care being taken that they are not raised so high as to prevent the complete filling of the tidal basin by each flood.

Additional borings would of course be made before definitely fixing the width between the jetties, as it is possible that beds of material incapable of removal by natural scour may exist at such moderate depths that the half-mile gap should give place to a considerably wider one, a question which will doubtless turn mainly on the quantity of materials that may require to be excavated by dredging.

No change of this character and for this purpose, if judiciously made, would materially alter the estimated quantities of materials needed for the construction of the works.

The volume of water, a little more than thirty-six hundred and fifty-five millions of cubic feet (3,655,374,296), which is supposed, in the foregoing discussion, to pass out through the gorge of the harbor on each ordinary ebb-tide, is believed to be less than the actual outflow of one tide.

Computations, in all respects similar to those given above, have been made on the supposition that the volume of outflow during each or ordinary tide, is 4,834,000,000 of cubic feet, which is believed to be somewhat in excess of the actual outflow.

The computed hydraulic radius in the gap between the jetties, is the same in both cases, which was to be expected, for the reason that we have only the calculated slopes and mean velocities to deal with, and that these vary with the volume of flow through the same section. The actual slope and velocity may be assumed to lie somewhere between those deduced in the two cases, and therefore, to correspond to the deduced hydraulic radius. These theoretical results are of practical value only when they point to bottom velocities possessing a scouring power of sufficient intensity to maintain the new channel. In the case under discussion, they theoretically satisfy that condition. Greater velocities could, of course, be established between the jetties by raising them higher, and in the seaward profile by extending them further out upon the bar.

It is quite likely that there would be an advantage in locating the sea-ends of the jetties about one-fourth of a mile to the southward of the points indicated on Plate I. This would place the center of the half-mile gap at the point Y,

where the sea end of the south jetty is placed in the drawing, and would turn the axis of the new channel more away from the prevailing storms which come from the northeast. The jetties in these positions are shown in Plate I, by heavy broken lines. It is not intended in this project to fix definitely either the length or the height of the jetties, or their precise location or distance apart, but to submit a general plan of improvement by means of submerged jetties that shall have their crests, throughout those portions which cross the thread of the current, at a height corresponding to the least width of the gap between them, the objects sought by this method being to lessen the first cost of the jetties, and to obviate the necessity of their subsequent extension.

The foregoing discussion will be revised, if necessary, in a supplementary report, as soon as the actual velocities have been ascertained by observation, and the requisite borings have been made.

CONSTRUCTION AND ESTIMATES.

The jetties to which the following estimates apply are those last discussed, located on the lines CX′ and DY′, Plate I. The varying heights to which they rise above the bottom are shown by heavy parallel hatching in longitudinal sections CXX′ and DYY′, Plate II.

Their sea ends for a length of 3,000 feet on the north jetty and 3,500 feet on the south jetty have their crests at the level of half flood of spring-tides, or 3 feet above mean low-water.

The total length of the north jetty is 8,480 feet, and that of the south jetty 13,040 feet. These are theoretical lengths. In practice it will probably be found necessary to give some additional length. They are to consist of a superstructure of riprap stones with rather low side slopes resting on a mattress of fascines 2 feet thick.

The slope on the exterior faces of the jetties will be 1 upon 2 throughout their entire length. On the interior faces it will be 1 upon $1\frac{1}{2}$, except on the sea ends, where, for a distance of about half a mile, it will be 1 upon 2.

For the north jetty the minimum width on top is 15 feet. This is in the lowest portion where it crosses Beach Channel. From that point outward, the width increases to 24 feet, which is adopted for that portion which rises above mean low-water level.

The south jetty has a minimum width of crest of 12 feet where it crosses the main channel, at depths varying from 10 to 15 feet below mean low-water. Thence outward the width increases to 24 feet for the highest part, as in the case of the north jetty.

It cannot perhaps be safely assumed that beds of clay which may be encountered near the surface are sufficiently firm to resist the weight of the works, without considerable subsidence. Where such beds, however, are overlaid by a thick stratum of sand, or a mixture of sand and shells, no great disturbance may be expected.

Where the jetties are constantly submerged, they will not exert a pressure upon the mattress foundation exceeding 91 pounds per square foot for every foot in height, to which must be added, where the work rises above low-water level, about 59 pounds more for each foot in height during the time they are out of water. This takes no account of any lateral distribution of weight, which must in a greater or less degree take place in riprap constructions.

There being only two points where the actual pressure upon the bottom will approach near to one ton per square foot, while it will generally fall below one-half ton, it is believed that no settlement or disturbance of a very serious character will be likely to take place. At the two points referred to, in the main channel, both weight and cost could be reduced by replacing a portion of the hearting of the jetty with mattresses similar to those used for the foundation, as shown in Fig. 2, Plate II, care being taken to keep the wood well inside the riprap, so that after the voids in the latter have become filled with sand, it would be safe from the ravages of worms. During the progress of work the voids could be filled at moderate cost by pumping sand from the bottom near by.

Riprap suitable for the entire work, except the facing of the sea ends of the jetties, can be procured for $3.75 to $4.00 per cubic yard, measured in the jetties. The stone for facing should be rather large, and will cost $5.50 to $6.00 per

cubic yard. The foundations of mattresses or poles, can be laid for about $1.00 per square yard.

Twenty to twenty-five per cent. would be a fair estimate for additional riprap, required to compensate for subsidence.

A liberal allowance of dredging and raking in the new channel, in material not susceptible of removal by the scour of the current, would be $150,000.

Due account being taken of contingencies, the total cost of both jetties may be stated at $1,500,000 to $1,800,000.

EXPLOSION OF A WESTERN RIVER STEAMER.

By JOHN W. HILL, M. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

On the night of May 17th, 1876, the steamer Pat Cleburne, of the Evansville, Cairo and Memphis Packet Company, a vessel plying between Evansville and Cairo on the Ohio river, exploded three of a battery of four boilers, completely wrecking the vessel and killing and injuring more than twenty people.

The steamer left Evansville on the afternoon of the fatal day, and between ten and eleven oclock P.M., made a landing at Shawneetown, several miles below the confluence of the Wabash and Ohio rivers: about one hour before midnight, the boat rounded out from this port and pursued her course down the Ohio. When within a distance of two and a half to three miles from Shawneetown, the steamer was hailed to come alongside by the Arkansas Belle, a vessel of the same line lying to at Coles, a landing said to be three to three and a quarter miles below Shawneetown.

The Cleburne steamed down to the Arkansas Belle, rounded in and drew up alongside. When she came abreast of the Belle, and about six feet separated therefrom, the port and two central boilers exploded with terrible violence, killing among others the master and chief engineer.

From the surviving officers of the wrecked steamer, and the officers of the Arkansas Belle, the following facts are obtained: The Cleburne was running with a pressure of one hundred and twenty-five pounds steam (considerably less than the U. S. certificate of inspection allowed) and blowing through the feed water heater at time of explosion—the furnace doors were opened to shorten the fires—the doctor (feed pump) was working at usual speed—the engines were stopped or slowed whilst rounding in. When the boats were nearly abreast the port engine bell rang to go ahead—the chief engineer who had previously hailed the officers of the Arkansas Belle from the engine room window, stepped back on the foot board—dropped the rods—opened the throttle, and the explosion promptly followed. The starboard boiler was uninjured except the breakage of connections, but after the explosion it was found rotated fore and aft on its seat. This boiler was shortened after the explosion, and set up on the steamer Idlewild for port duty. About fifteen months after the explosion occurred, the facts above were given to the writer, with instructions from the steamboat company to investigate the explosion and report upon the probable cause. No effort was spared by the officers of the company to arrive at the facts, and every facility was offered to make the inquiry as searching as the limited materials permitted.

According to the certificate of inspection, the machinery of the Pat Cleburne consisted of two non-condensing engines each $20'' \times 84''$ cylinder; steam was conveyed to these through $5''$ copper pipes; the doctor drove two cold water pumps and two hot water pumps, each $5''$ diam. $\times 12''$ stroke. The boilers, four in number, were of the return flue variety, each $24'$ long $37''$ diam. with $2—14''$ flues, the clear space between flues, and between flues and shell was $3''$. The shell courses were of $\frac{5}{16}''$ iron and the flues of $\frac{1}{4}''$ iron. All shell joints were single riveted. The after ends of the boiler were concave, and the mean length of flues about $22'$

6″. Three boilers were furnished with free safety valves, and one boiler with a lock-up valve; each boiler had three Mississippi gauge cocks and a low water gauge; fusible plugs were inserted in the fire courses and after ends of flues of each boiler. The evaporation was collected in a large cylindrical steam drum lying athwartships and connected by 12″ legs to the second after course of boilers. The steam pipes, (two), connected with the steam drum, midway between the first and second legs and the third and fourth legs. Under the boilers, and directly opposite to the steam drum, lay the mud drum; this was connected to the boilers by 12″ legs. The hot water pumps delivered the feed through direct copper pipes to the mud drum.

The boilers were built in Cincinnati during the year 1870, and at time of explosion had been in use less than six years. According to the U. S. inspector's certificate, issued about five months previous to the disaster, the limit of working pressure was fixed at one hundred and forty pounds by gauge, and every detail of boilers and attachments complied with the U. S. Treasury regulations.

The exploded boilers were literally torn to fragments, and no portions of shells or flues were in existence at time the writer began the investigation. The fusible metal in the safety plugs was Banca tin, and when found, nearly all of these were melted out; but as the wreck burned to the water's edge, within a very few minutes after the explosion, the probability is that these plugs were melted out after, and not before, the explosion.

The officers in charge of the Pat Cleburne, were of the best on the lower Ohio, and the chief engineer was reputed without a superior in the management of steam boat machinery. After commencing the investigation, the following facts were obtained: from the master of the wharf boat at Shawneetown; that he was on the vessel, conversed with the engineer, and saw him test the water level in the boilers within fifteen minutes of the explosion: from the second engineer of the Cleburne who was asleep in the "Texas" when the boilers let go; that he was on the boiler deck within an hour of the explosion, and no known derangement of doctor or boilers existed, except a slight leak in the second or third roundabout joint upon the side of one of the central boilers: from the master of the Arkansas Belle, who was on the starboard guard of his vessel when the Cleburne rounded in; that the port wheel of the wrecked steamer made a revolution or partial revolution before the boilers let go; indicating that the cam rods had been dropped in gear, and the throttle opened to give steam to port engine; and that the piston had begun its stroke. By way of explanation it should be remarked; that when a river steamer is under way, and a necessity for stopping occurs, the throttle valve is but partially closed, the cam rods unhooked, and the valves set to blow through.

Thus the surplus steam, instead of wasting through the safety valves, is blown through the cylinder into the heater, and utilized to elevate the temperature of the feed water to the boilers.

The facts enumerated, from the surviving officers of the wrecked steamer, the officers of the Arkansas Belle, the superintendent of the steamboat company, and the inspection certificate, were the basis of examination. In the West, and, so far as the writer is aware, in the East also, when a steam boiler explosion occurs, the first step is to secure a scape goat to carry the burden of blame: if the engineer in charge survives the disaster, he is usually "honored" with the appointment; if he is killed, sympathy overbalances public prejudice, and the excoriation is discharged in some other direction.

In the case of the Cleburne, however, the very excellent discipline maintained by the steamboat company, together with the known qualifications of the officers of the steamer, and especially the fact that the unfortunate chief engineer was above suspicion of incapacity or negligence, had an effect to stultify wild speculation on the cause of the explosion.

The facts obtained support the following assumptions:

First. No known defect existed in the boilers or feed water machinery of the Cleburne when she rounded out from Shawneetown.

Second. Upon leaving Shawneetown,

the Cleburne steamed up to the usual running pressure; and the signal to come alongside the Arkansas Belle was unexpected: (shortening fires and blowing off were resorted to, to control within safe limits, the steam pressure).

Third. No evidence of danger on the Cleburne had presented before coming alongside the Arkansas Belle: (the firemen having opened the furnace doors and walked out on the port guards, and when nearly abreast, the chief engineer of the Cleburne came to the engine room window and cheerily hailed the officers of the Belle).

Was low water the cause of the explosion? When the Cleburne left Shawneetown, we are informed, the usual level of water obtained in all the boilers; from this port to the meeting with the Arkansas Belle, not more than fifteen minutes elapsed, during which time no steam was blown off save through the engines. Neglecting the leak, which we are informed was insignificant, then the reduction of water level in the boilers (assuming a total failure upon the part of the feed pumps to supply during the interval) would be that due to evaporation alone.

The aggregate heating surface to each boiler is taken as 325 superficial feet, and maximum evaporation per hour per square foot of heating surface as *six pounds;* and maximum evaporation per boiler for fifteen minutes $\frac{325 \times 6}{4} = 487.5$ pounds, or 8.75 cubic feet at temperature of 353 Fahr. This evaporation corresponds to a reduction of water level of less than one and one half inches; the usual level of water over the flues was *four to five inches.* All evidence went to prove that no failure to supply the boilers occurred prior to the explosion; the doctor was simply a small beam engine, with a plain slide valve; driving four pumps—two piston pumps for cold water, and two plunger pumps for hot water. The cold and hot water pumps were in duplicate; in the event of failure of one pump, the other was of sufficient capacity to supply the boilers. As against a sensible reduction of water level in the boilers during the fifteen minutes run—whether from failure of the "doctor" to supply, or from any other cause—the frequent examination of the water level is "second nature" to the experienced engineers; hence the writer is unwilling to believe that a person of the experience and known capacity of the first engineer of the Cleburne, with the doctor and water gauges directly under his eye, would fail to detect a fault in the working of the one, or test the other, during the run from Shawneetown to the meeting with the Arkansas Belle. Assuming, however, that no water was supplied to the boilers after leaving Shawneetown, then the reduction of water level, by evaporation alone, could not have been sufficient to uncover the flues. In fact, the water over the flues at the time of explosion could not have been less than *two and a half to three inches,* quite enough for all purposes of safety. The blow off, or mud valves, as they are termed on the Western rivers, closed under pressure, and could have been opened only by manual effort; no evidence offered to show that these valves either leaked or were opened, hence it is reasonable to conclude that no water left the boilers by this outlet.

The leak, already noted in one of the central boilers, was in a roundabout seam forward of the bridge wall, and had been noted from time to time by the chief engineer for several days. From the statements of the colored firemen who survived the disaster, this leak was due to defective caulking of the overlap, and was no evidence of weakness in the boiler. (Boilers frequently leak at the riveted joints, and a new boiler absolutely free from seam leaks is a rare circumstance. But a leaking joint in an otherwise sound boiler, is no cause for alarm; the caulking that makes a joint tight under pressure adds nothing to the pronounced strength of a boiler, and the only effect of a seam leak would be to impair the economy of performance, and impose an increased duty on the feed pump). This leak was in plain view from the front of the boilers, and could be seen by the fireman every time the furnace doors were opened; these *were* opened and fires banked within two to three minutes of the time of explosion, and it is not very probable that an increase had taken place in the leak, without the fireman observing it. The after end of each flue contained a fusible plug, and at this point the hot gas passing forward through the flue is at the maxi-

mum temperature; the plugs were inserted in the crowns of the flues, where the collection of scale is a slow process; and it is very unlikely that of eight independent plugs supposed to be in the same horizontal plane, not one would have melted and given an alarm, had the water level fallen below the crowns of the flues before the explosion.

When found, the fusible metal in some of these plugs was melted out, but the fragments of the boilers lay on the wreck of the vessel while it burned; and there can be no doubt that these plugs were fused in the raging fire which promptly followed the explosion.

When the boilers of the Cleburne ruptured the fusible plugs were intact; for the peculiar whistling sound, as the steam and water rushes through the orifice in the plug, could not have escaped the attention of the engineers and firemen on watch. Let it be supposed, however, that the water level had fallen so low as to uncover the crowns of the flues and melt the metal in the plugs (as it has been asserted in connection with this disaster); would this have been a sufficient cause for the explosion? Evidently not, if fusible plugs are possessed of any virtue: for the plug, or rather the core of the plug, is not supposed to melt until the crown of the flue is uncovered, and heated to a temperature of 420° Fahr.; and as the fusing and blowing out of the core is only intended as a timely warning against danger, it follows that the melting of these plugs would be no argument in behalf of low water as the cause of the explosion. As a further argument against low water as the cause of the explosion on the Cleburne: in rounding in the vessel listed to port, thus elevating the boilers to starboard, and low water, if it obtained at all, obtained to the greatest extent in the starboard boiler; *this boiler was wholly uninjured,* and is now in daily use on another vessel of the same line.

Without discussing "low water" as a probable cause of explosion in boilers of this class, set and fired as were these boilers; the writer would suggest that low water was *not* the cause of explosion in this instance, and all the facts appear to sustain this view.

Examining as to the probability of explosion by defects of materials, improper construction or deterioration from use; we find that the boilers (four in number) were all made at the same time, of the same brands of iron, of precisely the same dimensions, and had been worked together for six years, under like conditions. During this time they had been inspected many times, at different ports, by different inspectors, and had defects of materials existed, they would have, in all probability, been detected before the explosion. Whilst there is no doubt of the reckless manner in which boilers are put together being a fruitful source of explosions, no evidence was offered to show that the boilers of the Cleburne were not well built; and if the surviving boiler is an index of the workmanship, they were in this respect considerably above the average. The precise condition of the boilers at time of explosion is not known, except they had been carefully washed out a few days before. But as the boilers had always worked together, and resisted the same strains, and destructive action of fire and water, it is reasonable to presume that the unexploded starboard boiler was no better than the others. This boiler was opened after the accident, and a careful examination revealed no special or dangerous deterioration.

It has been suggested that over-pressure was the cause of the explosion. Under the certificate of inspection the boilers of the Cleburne were limited to 140 pounds by the gauge; but at the time of the explosion, or more correctly a few minutes before, the pressure was 125 pounds; the last inspection was made less than five months prior to the accident: and under the U. S. Treasury regulations the working pressure is taken at one-sixth the tensile strength of plates, and the proof pressure at one and one-half times the working pressure: hence, the proof pressure of these boilers, according to the inspector's certificate, was 210 pounds. It is scarcely possible that their strength was diminished *forty per cent.* during the last five months of use. It might be supposed that the steam guages were unreliable, and failed to indicate the true pressure, which was considerably higher than indicated by the gauge. But from all the evidence furnished the writer, the pressure that ruptured the boilers was less than that at

which the safety valves were set to blow: this was one hundred and forty pounds, and the valves were frequently eased on their seats to insure prompt action.

As the writer understands the term, over-pressure was not the cause of the explosion; that the strains at time of rupture were in excess of the strength of the boilers is evident; but that the steam pressure steadily increased until the strains were in excess of the resisting powers of the boilers is scarcely possible, in view of the testimony of the engineer's assistant, and the surviving firemen, that the pressure was, within two or three minutes of the explosion, one hundred and twenty five pounds, with furnace doors open and fires banked.

Without adverting to other improbable theories of explosion, as applied to the ill-fated Cleburne, the writer will endeavor to establish what, in his opinion, was the cause, in accordance with the facts related. When the steamer left Shawneetown, the "regimen" of the boiler was calculated for a long run. The boiler capacity of river steamers to reduce dead load is usually a minimum, and active firing is frequently resorted to, to maintain a running pressure. But the flow of steam out of the boilers, and the flow of water in, is usually correspondingly uniform, and no evil effects are liable to follow forced firing.

When the Cleburne was hailed by the Arkansas Belle to come alongside, the condition of fires and steam pressure were unfavorable to a stop, and the furnace doors were opened and fires banked. But the time elapsing from receiving the signal, to its coming alongside the Bell, could not have been more than four or five minutes; and the time elapsing between the banking of fires and the explosion not more than two or three minutes. Upon reception of the signal to stop, the engines of the Cleburne were slowed; and whilst rounding in, the use of the wheels would be irregular, and chiefly confined to the port wheel; and when the vessels were nearly abreast the port wheel was stopped entirely for an interval of several seconds, during which time the vessel drove on by momentum. In warping in a spurt from the port wheel was necessary to avoid a bow collision—the port engine was started—when the explosion of the port and two central boilers almost instantly followed. Previous to the rupture of the boilers the steam and water had been heated to a temperature of 353° Fahr., and the iron of the under courses and flues to a temperature somewhat in excess of this. The walls of the furnaces were glowing from the active firing and the circulation sufficient to prevent overheating of iron or water; directly the speed of engines was slowed the rapid ebullition in the boilers was checked by the increase of pressure, and whilst the flow of feed water into the boiler may have been unchanged, the flow of steam out of the boiler, for a brief period of time ceased nearly, if not quite altogether. The natural result of this would be to reduce the circulation from previous activity to a state of partial quiescence, and localize the heat. The capacity of the water to receive heat and vaporize would be temporarily diminished, and the iron of the under courses quickly heated to a temperature sufficient to repel the superincumbent water from the plates. This temperature is variously estimated from 380° to 430° Fahr., hence we accept a mean of 405° as applicable to the iron in the boilers of the Cleburne; then an addition of 50° Fahr. would anticipate the condition of plates necessary to perfect repulsion. The previous active fires in the furnaces; the unexpected stop; the brief interval between receiving the signal to stop, and coming alongside the Arkansas Belle, were conditions favorable to the repellant action. Without entering into a discussion of the theory of repulsion, the *rationale* of which is well understood by steam engineers, the writer would suggest that directly the repellant action occurs, the iron of the boiler instead of acting as a vehicle of transmission of heat, becomes as it were a receiver of heat, and the temperature of the plates is rapidly augmented by the impinging hot gas. It is assumed, in the case of the Cleburne, that the repellant action occurred at a time when the engines were stopped, and the flow of steam from the boiler at a minimum, or checked entirely. At this time the circulation was sluggish, and ebullition slow and irregular. Meanwhile the storing up of heat in the iron of the shell went on until an unknown tempera-

ture was attained; no increase of pressure was indicated by the gauge, and no appreciable variation was noted in the water level; the fires were banked and furnace doors open, and so far as the engineer could qualify, every precaution had been taken to avoid danger. The port engine bell was rung to "go ahead"; the engineer dropped the cam rods, opened the throttle, and the piston began its stroke; the flow of steam to the engine reduced the pressure in the steam drum and steam room of the boilers, sensible heat became latent with a quick vaporization of a portion of the water. The reduction of temperature of the water, and the return to the highly heated plates, were instantly followed by the production of a comparatively large volume of steam which, in seeking to escape to the surface and vaporize, carried the water with it and delivered it as a projectile against the limiting surfaces of the boilers.

THE HYDROLOGY OF THE MISSISSIPPI RIVER.

REVIEW OF REPORT BY HUMPHREYS AND ABBOT.

By JAMES B. EADS, C. E.

Written for Van Nostrand's Magazine.

As the report on the Mississippi river made by Generals Humphreys and Abbot in 1861, has been recently republished by the Government, and as it contains certain grave errors touching the navigation of the river and the reclamation of its alluvial basin, I desire to expose them, and to show that many of the statements made by the authors of the report are not sustained by the facts to which they refer. If the reader will follow me attentively, I promise to demonstrate, to his entire satisfaction, the utter absurdity of these statements.*

It does not interest the general public to know whether the quantity of sediment carried by the water of the river, is adjusted by the rate of its current or not; or whether the real bed on which rest its moving sand bars, is of recent, or of ancient geologic stratification, or whether it wears rapidly or slowly under the action of its current, unless these questions are known to have an important bearing upon the commercial and agricultural prosperity of the Valley of the Mississippi. When this is known to be the fact, the scientific interest in them is completely dwarfed by the overwhelming practical bearing which they have upon great national interests. It is for this reason that I select your widely circulated journal as the surest means of thoroughly reaching the intelligent readers of the country, rather than to attempt, through the less extensively circulated records of any of the scientific bodies of which I am a member, an exposition of the dangerous errors advanced by Humphreys and Abbot.

THE RELATION BETWEEN THE CURRENT AND THE SUSPENDED SEDIMENT.

In 1874, I stated in a pamphlet, that the chief portion of the sediment discharged by the river into the Gulf is carried in suspension, and "that the amount of this matter, and the size and weight of the particles which the stream is enabled to hold up and carry forward, depend wholly upon the rapidity of the stream, modified, however, by its depth."

General Humphreys immediately afterwards said,* this statement is "in direct conflict with the results of long continued measurements made upon the quantity of earthy matter held in sus-

* In 1874 I proved to the satisfaction of the Congress of the United States, *by the data contained in this report*, that the theory of bar formation at the mouth of the Mississippi advanced by its authors, was totally wrong, and thus secured for the river an unobstructed and open outlet to the sea through the bar at South pass. It is needless to say that the predictions made by General Humphreys regarding the re-formation of the bar in advance of the jetties, have not been realized. This paper is intended to expose other erroneous theories advanced in the same report, and which stand in the way of a correct system of improvement of the entire river, and which are declared to be conclusively demonstrated by patient scientific and experimental investigation.

* See Executive Document 220, 43rd Congress. Also last edition of Report on the Mississippi River, page 674

pension by the Mississippi river at Carrollton (near New Orleans), and at Columbus (twenty miles below the mouth of the Ohio), one of the chief objects of which was to determine this very question, whether any relation existed between the velocity and quantity of earthy matter held in suspension. These results prove that the greatest velocity does not correspond to the greatest quantity of earthy matter held in suspension; on the contrary, at the time of the greatest velocity of current at Carrollton, the river held in suspension but little more sediment per cubic foot than when the velocity was least." *

These results when correctly interpreted prove precisely the contrary of the idea here conveyed by General Humphreys. He says that my statement is in direct conflict with them, and then proceeds in effect to tell us, that there is no relation between the velocity of the current and the sediment carried in *a cubic foot of water*, which is a very different thing, as the reader will soon see.

Gen'l Humphreys evidently means to convey the idea that the most rapid current carries but little more sediment than the least, when in fact by his own tables, it carried more than twenty times as much as the least current at Carrollton, and more than forty times as much at Columbus.

They use the terms "a cubic foot of water" and "the current," as expressions having one and the same meaning; whereas the current per second represents the force due not to *one* only, but to an immense number of cubic feet of water passing, in each second of time, by the place where the current is measured; and it is the total sediment suspended in this immense number of cubic feet that should be compared with the rate of the current per second.

One of the chief objects, we are told, was to determine "whether any relation existed between the velocity and the quantity of earthy matter held in suspension." In what? *In a cubic foot* of water, or in the whole river? Certainly in the latter, for the quantity in a cubic foot is of no practical value except as a means to determine its relation to the whole quantity.

They pushed their investigations however only to the extent of trying to find the relation between the current per second and the sediment in a cubic foot. Failing to discover this, for they proceeded no farther, and supposing that they had solved a problem in which they had neglected two essential elements, they announced their astonishing discovery that no relation whatever exists between the rate of current and the quantity of sediment suspended by it; or, in plainer English, between *cause* and *effect*.

This question could only be solved by bringing the elements of *space* and *time* into the computation for the sediment, just as they are brought into the current measurement, that is, by comparing the mean velocity *per second* with the total weight of sediment suspended per second. They, however, compared the mean velocity in every instance with the mean sediment contained in but a single unit of the river's volume, and they not only published the results of this meaningless comparison, as *a proof* that there is no relation between the rate of current and the quantity of sediment, but they have founded unsound theories upon this error, and have officially advised a dangerous system of river treatment based upon it.

I will now show *why* they should have compared the current, per second, with the total quantity of sediment passing by their point of observation in the same unit of time. To make this easily understood by the general public, compels me to state much that will be commonplace to the scientific reader.

Motion cannot occur in matter without an expenditure of force. The transportation of sedimentary matter in water, can, therefore, only result from an expenditure of force, and only by supplying the requisite amount of force, as it becomes exhausted, can these matters be lifted up and kept from falling back to the river bottom. Being heavier than water, it is just as impossible to uphold them in it without force, as it is to raise chaff in the air, or sand and dust in a whirlwind without it. The current caused by the river flowing from a higher to a lower level supplies this force.

* See last edition Mississippi River Report, page 138, and Appendix D.

The investigation of all questions relating to the expenditure of force, belongs to that branch of science called *Dynamics*, and in all such problems, whether they relate to a treadmill, or a steam engine; to the tiniest ripple, or the grandest river; to a grain of sand as it moves onward to the sea, or to the most majestic planet that pursues its pathway in the heavens, each and all involve the consideration of four distinct elements in their solution; and unless each one of these be duly considered no assumed solution of the question can be worth the paper on which it is made, except perhaps to "point a moral."

These elements are, first, *force*, second, *matter*, third, *space*, and fourth, *time*. Gravity and pressure are examples of the first element, and one of these, gravity, constitutes the first factor in our problem. The term volume, or mass, is used to indicate the quantity of the second element, while the term speed or velocity embraces the last two elements, and indicates the space through which the force acts, and the time involved in the action.

The amount of force expended can only be ascertained by knowing the weight or pressure exerted, the space through which it acts, and the time occupied in such action.

The relation of these four elements to each other may be illustrated by suspending two equal weights from the ends of a lever with equal arms, supported at its middle. While at rest they present simply a statical problem, in which force, matter and space alone, are involved. When in motion, however, the other element, time, necessarily enters into the problem. If motion be imparted to the weights, and one sinks towards the earth, the other will be raised through a space exactly equal to that through which the other falls, and in the same time in which the other falls. The velocity and mass of the descending weight gives the measure of the force expended. This force can only be determined by these three elements, first, the weight, second, the space through which it moves, and, third, the time required to move through the space. *The work done* consists in its raising the other *weight* through the same *space*, and in the same *time*. Therefore the force expended will be precisely the same that is required to raise the same weight, through the same space, in the same time. Hence it is an axiom that "The work done must bear an invariable quantitative relation to the amount of force expended."*

If the point of support of the lever be moved from the center toward one weight until the latter will balance one only half as heavy, it will then be found that when the large weight descends in one unit of time through a certain space, the small weight will have been raised through twice that space in the same unit of time, and therefore, the small one will have moved with twice the velocity. Hence, if we raise a weight through twice the space, in the same time, we must either double the force, or lift but one-half the weight. If we reverse the motion of the weights, and the smaller one descends, we illustrate the fact that by doubling the velocity, half the force will lift twice the weight.

In the steam engine the pressure of the steam takes the place of the pressure or force exerted by gravity. To determine the power of the engine we must have, first, the pressure upon the piston, second, the space through which it moves, and third, the time occupied in its movement. If the same pressure be maintained per square inch in each of two cylinders, and the velocity of the piston in one be twice as great as in the other, the more rapid one will develop as much power as the other with half the area of piston; just as half the weight on the doubled length of the lever arm can develop the same amount of force as the whole weight, because it will then move with twice the velocity.

The power of a waterfall is estimated by the same three elements. The weight of the water falling in one minute of time and the number of feet of space through which it falls in the time, are multiplied together, and when divided by 33,000 foot pounds, the quotient will represent the horse power of the waterfall or head of water; a horse being supposed to be able to raise 33,000 pounds, one foot high, in a minute of time.

It is unnecessary to point out by farther illustration the fact that these three elements, matter, space, and time, are inseparably related in any investigation to determine either the amount of force

* Mayer.

expended or of work done. I need only add that no matter how intricate the machinery, or secret the medium through which moving bodies transmit their forces, these three elements are as absolutely requisite to determine the amount of the force expended, or the work done, as the depth, width, and length of a rectangular box are, to determine its capacity ; and no matter how occult may be the relation between them, it is nevertheless as indissoluble, complete and perfect as in this simple illustration.

The work performed is precisely equal to the force expended when operating any steam, water or other motor, but the work *practically* considered is of two kinds: one of which may be called profitable or visible work, and the other unprofitable or invisible work, the latter being that part of the force which is expended in overcoming friction, back pressure, atmospheric resistance, radiation, &c.

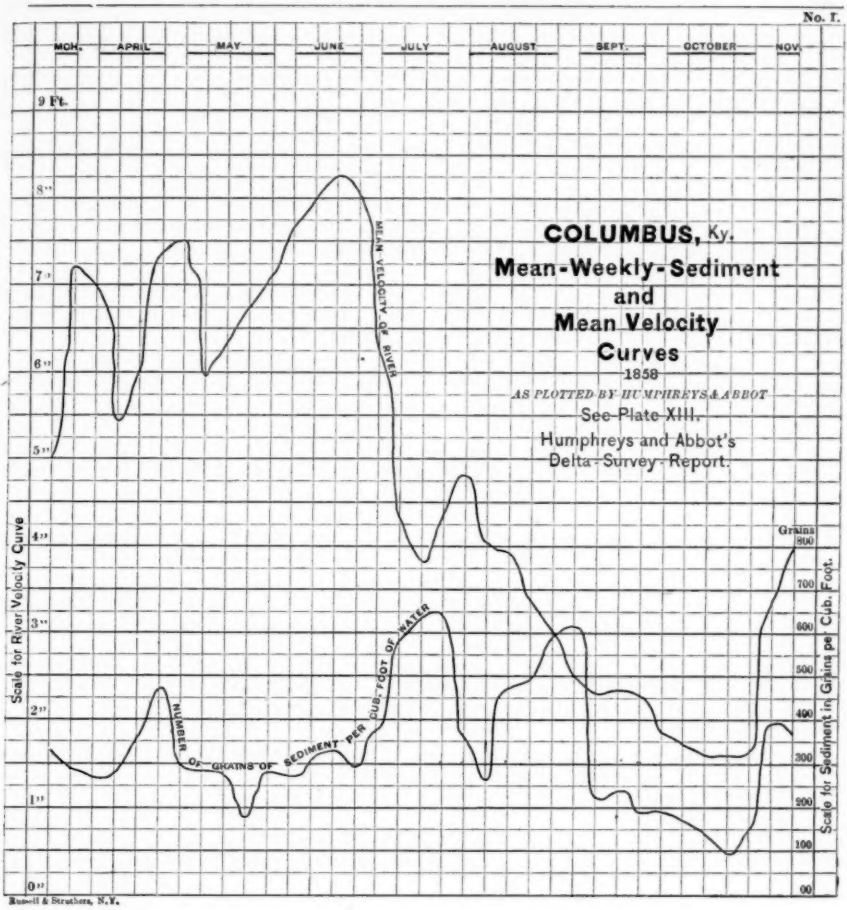
The work done by the force which the Mississippi River expends we may, for the sake of illustration, also divide into two kinds, and call the first, invisible, or unprofitable work, among which we may class the overcoming of the friction of the bed of the stream, the friction among the particles of water, the resistance due to the irregularities and bends in the channel, the atmosphere, &c., leaving to be considered, as the visible or profitable work, the transportation of its immense burden of sediment. The problem we are considering and which these gentlemen claim to have determined, is the relation which the current, or force, expended by the river bears to this great burden of earthy matter.

Let us suppose a railway train be used in transporting grain, and that we wish to determine the relation between the force (or coal) expended, and the quantity of grain carried ; we would carefully ascertain the total coal burned in some definite *time*, for instance, in one hour, and also the *total weight* of the grain carried in that hour, and likewise the *space* over which it was carried during that hour. We would then be able, by comparing the total coal with the total weight, to declare absolutely that so much coal or force expended, was equal to the carrying of so much grain a certain distance in one hour, and the relation between the force expended and the work done would be so expressed.

In such investigation we would have 1st, force (the coal) ; 2d, matter (the load of grain); 3d, space (the distance the load is carried); and 4th, time (the hour during which it was carried). By repeat- the measurements under similar conditions, but with different quantities of time, space and weight, this relation between force and work would appear constant and inseparable. An instructive comparison could only be made, either between the *totals* of the force and work, or between their respective *units*, and in either case *time* and *space* would be indispensable elements to be considered. But if the total coal be only compared with the weight of a *single bushel* of the grain, and no note be taken of the *space* through which it was carried, nor of the total number of other bushels that were carried in the same *time*, the comparison would have no significance whatever. A diagram to represent such a comparison, as an ultimate solution of the question, would not only be meaningless but absurd; yet it would be precisely similar in principle to the diagrams which Humphreys and Abbot represent on plates XII and XIII of their report, where the current per second is contrasted with the sediment found in a single cubic foot of water. An accurate fac simile of plate XIII is herewith shown. (See diagram No. 1.)

If the mean current at Columbus was six feet per second, an entire section of the river six feet long must have moved at that place and time through the space of six feet, and the force expended was, therefore, the entire force due to the motion of this whole section during that second.

The mean current given in feet per second, is, therefore, an exponent of this whole force, and if it be six feet per second, it can only be intelligently compared with the total sediment carried in an entire section of the river six feet long, and not with that in a single cubic foot. If we multiply the cross section of the river in square feet by the current in lineal feet per second, the product would be the number of cubic feet in the section, and these multiplied by the number of grains of sediment in one foot,



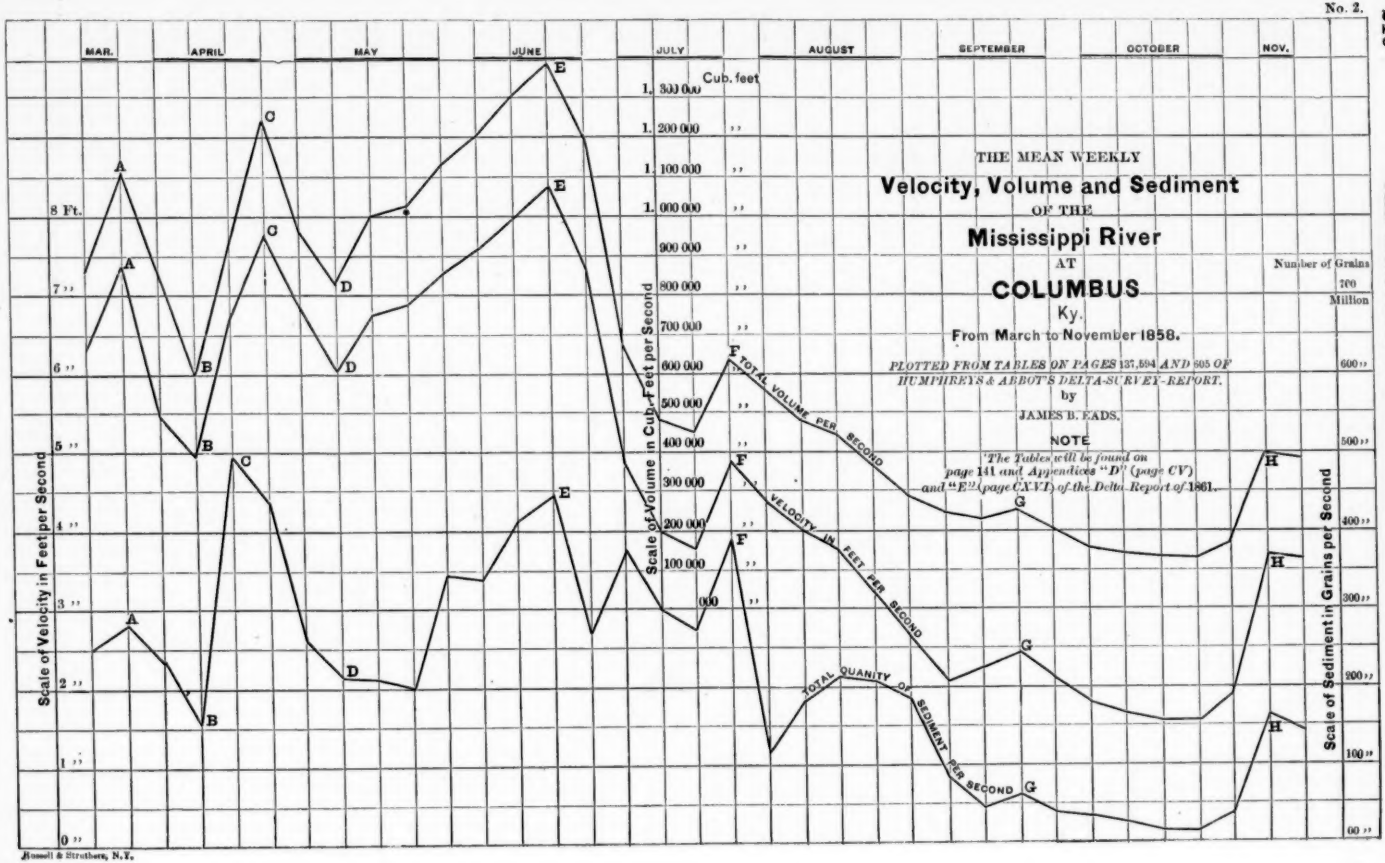
would give the proper amount for comparison with the current.

As the work done and the force expended must be precisely equal, it is evident that the three elements, namely, *matter, space and time*, are as necessary to determine the amount of *work done*, as they are to determine the amount of force expended.

In appendix D of their report will be found tables, giving in cubic feet, the daily volume of water flowing per second, by the velocity base or point where these measurements were made: These quantities were ascertained by multiplying the cross section of the stream in square feet each day with the mean velocity of the current at the time, in linear feet per second. The two absent dynamic elements, namely, *time*, (one second), and *space*, (the linear feet the river moved in one second), are thus included in these tables. By taking the average or mean weekly discharge in these tables, and multiplying it with the mean sediment in grains found each week in one cubic foot of water, given in the tables, we get the proper quantities of sediment to compare with the average rate of current per second.

Diagram No. 2 is prepared in this manner from precisely the same data which Humphrey's and Abbot used to prepare Diagram No. 1, except that in mine the absent elements, *space* and *time*, have been included as above explained. A

No. 2.



THE MEAN WEEKLY

Velocity, Volume and Sediment

OF THE

Mississippi River

AT

COLUMBUS

Ky.

From March to November 1858.

PLOTTED FROM TABLES ON PAGES 137,594 AND 605 OF HUMPHREYS & ABBOT'S DELTA-SURVEY-REPORT.

by

JAMES B. EADS.

NOTE

'The Tables will be found on page 141 and Appendices "D" (page CV) and "E" (page CXVI) of the Delta Report of 1861.

Russell & Struthers, N.Y.

third line is shown on my diagram which gives the mean weekly volume of discharge, by which the total weekly mean of sediment was ascertained.

If the relation between the current velocity and the quantity of sediment does not exist, as Humphreys and Abbot assure us, no correspondence or synchronism could be graphically shown between them on diagram No. 2 by any possible scientific analysis to which these data can be subjected. By their diagrams, none whatever is shown, because of their error.

An inspection of diagram No. 2 proves the existence of this relation in a way that admits of no dispute, and shows how remarkably sensitive the sediment is to any change of velocity in the current. This is particularly noticeable at each period when the current began to decline. The river rose and fell six times at Columbus while the observations were being made. These periods are indicated by the letters A, C, E, F, G, and H. The loss in velocity at each of these six periods, during the eight months, is invariably and *immediately* marked by a corresponding reduction in the quantity of sediment. No one can look at these two diagrams, made from the same tables and to determine the same question, without feeling assured that

"Some one has blundered."

A diagram made in the same manner from the Carrollton observations will show an equally striking evidence of the intimate relation between the rate of current and the quantity of sediment, which has been so persistently and dogmatically disputed.

The error made by Humphreys and Abbot when investigating the results of their experiments at Columbus and Carrollton, consists in supposing they were comparing a definite exponent of the *force* with a corresponding exponent of the *work*, when, in fact, the elements of *space and time* were wholly absent in the exponent of the work; and not only were these neglected, *but only one single unit of the third element of the work* was taken as the corresponding exponent to compare with the force.

Suppose we should attempt to show the relation between a certain quantity of grain, and the capacity of a rectangular box which it had exactly filled. Having ascertained the number of cubic inches of the grain, what relation could we hope to show between this quantity and the capacity of the box, if we compared it with only one single inch of the length of its bottom? Not only would we be ignoring the total length of the box, but we would also be neglecting the two other factors of the problem, namely, its width and its depth, and the comparison, therefore, would be utterly unintelligible. Such a mistake would be inexcusable in one who had barely entered on the threshold of geometry. The mistake made by Humphreys and Abbot is similar to this, and it is one equally unpardonable even in the merest tyro in the science of dynamics. Yet, relying solely upon this method of investigation, the Chief of Engineers of the United States army, to defeat the adoption of the present system of improvement at the mouth of the Mississippi river, actually prepared a letter which was read in the House of Representatives in 1874, and which referred to the subject we are discussing in the following language: "It is probably unnecessary for me to say here that, the statements which Mr. Eads has made in the pamphlets he has published concerning the conditions existing in the Mississippi river and at its mouth are the mere revival of old assumptions, which experimental investigation has long since shown to be utterly unfounded in fact."

Having clearly explained how their defective knowledge of the principles of dynamics led them astray, and having proved by their own testimony that they are clearly in error, let us now see to what absurd conclusions their unfortunate mistake carried them.

Referring to their experiments at Columbus and Carrollton they say on page 135: "An inspection of the preceding table must convince any one that the Mississippi water is undercharged with sediment, even in the low-water stage. A most important practical deduction may be drawn from this fact, namely the error of the popular idea that a slight artificial retardation of the current, that caused by a crevasse for instance, must produce a deposit in the channel of the river below it."

On page 417 this undercharged theory is repeated, as follows:

"A glance at the two diagrams is sufficient to demonstrate the falsity of the assumption, that Mississippi water is always charged with sediment to the maximum capacity allowed by its velocity."

Having exploded the "error of the popular idea" that cause and effect are related, we need not be surprised at this *undercharged theory*. And although we may have supposed that matter cannot move independently of law, and that neither an atom nor an avalanche can stir except in strict obedience to ordinances more fixed than those which swayed the Medes and Persians, we must be prepared to believe that the sediment of the Mississippi is an exception to this rule, for, having proved conclusively that its water is always undercharged, we are gravely assured on page 135, "If the water be undercharged, the distribution of sediment will follow no law, the amount at any point being fixed by the accidental circumstances of whirls, boils, &c." With such astonishing declarations as these, the reader will be partially prepared for the no less wonderful announcement that as the sediment will follow no law, the feeblest current can carry just as much of it as the most rapid current.

This statement will be found on page 684 of the last edition. It is as follows:

"In fine, these measurements upon the quantity of earthy matter, suspended in the Mississippi river, show that at no time has the water been so heavily charged with it that the current could not carry it along in suspension to the same extent as it did when the quantity of earthy matter was least; and they further show that *the current* of the Mississippi river, when most feeble, can carry in suspension the greatest quantity of suspended earthy matter found in it, to the same extent that it can carry the least quantity found in it."

I know of but one other statement concerning the wonders of this river that can compare with this one. In the last eighty years several cut-offs have occurred below the mouth of the Ohio, by which the channel was shortened about seventy miles. Based upon this fact, a distinguished writer has published the startling prediction that within a few centuries, two cities on the river, (Cairo and New Orleans) although now distant from each other one thousand miles, must, by this shortening process, inevitably be drawn together! By an inverse method of reasoning on these facts, he arrives at the interesting conclusion, that in some remote geologic period the Mississippi extended to Cuba! *

When pursuing a different line of investigation, distinguished engineers arrive at the equally astonishing conclusion, that the current of the Mississippi when most feeble can carry as much sediment as it can when most rapid, we may from the standpoint of common sense, safely assume that while the deductions, in each case, rest upon facts, the conclusions in both were arrived at by defective methods of scientific investigation.

If we examine these Carrollton and Columbus experiments we do not find this surprising statement about the power of feeble currents verified.

In the quotation, I have italicised the words "the current," to attract attention to the fact that no distinction is made between what *the current* carried and what *a cubic foot* of water carried. Diagram No. 2 shows what the current carried, while diagram No. 1 shows what was carried in *a cubic foot*. The one emphatically disproves this absurd statement, while the other furnishes no ground whatever for making it, because it conveys no idea at all of the relation between the current and the sediment.

At Columbus, the most feeble current carried but ten million grains of sediment per second, while during the third week in April, when the current was about four times as rapid, it carried 480 million grains, *or forty-eight times as much* as "when the current was most feeble." At Carrollton the current was most feeble in November, being but little more than a foot and a half per second, and then it carried less than 22 million grains, while in June, when the current was nearly three times as rapid, it carried 500 million grains, *or nearly twenty-three times as much* as when it was most feeble!

Dr. G. Hagen, Director General of

* Mark Twain.

Public Works in Prussia, and one of the most eminent engineers in Europe, in a recent criticism upon Humphreys and Abbot's theory regarding the distribution of velocity in flowing water, says :

"The young student of hydraulics is sometimes compelled to accept certain theorems as true and proven which, to say the least, are still doubtful; but he has as yet never been expected to receive devoutly a demonstration like this, and to regard it as a progress of science." This comment seems peculiarly applicable, likewise, to their conclusions regarding the relation between the current and the suspended sediment."

On the same page of their report from which the preceding remarkable extract is taken, is the following:

"This proposition, therefore, respecting certain velocities of current always carrying certain fixed quantities of earthy matter, and always adjusting those quantities according to its own variations of strength, is so entirely disapproved by facts that it will not be considered again."

In view of the fact that their own tables prove the utter fallacy of this statement, it is amusing to see the satisfaction with which it seems to be uttered.

It will be observed that all of these mistaken conclusions rest upon the assumption that the sediment found in *a cubic foot* of water, moving at different velocities, was a correct exponent of the ratio between the speed of the river and the burden it carried.

After referring to plates XII and XIII to prove that "the river is never charged to its maximum capacity of suspension" they declare (page 417)—"Hence if enough water had been taken from the river at the date of those floods (1851 and 1858) to reduce its velocity nearly to that of the lowest stage, no deposit in its channel could have occurred."

The highest velocity at Carrollton was 6.16 feet per second, and the sediment was then only 252 grains per cubic foot. In September the current had declined to 2.44 feet per second, while the sediment was 268 grains per cubic foot. These quantities were doubtless in view when the above declaration was made, because, as far as their "experimental investigation" had advanced it showed that a current less than 2½ feet per second actually carried more sediment *per cubic foot* than a current of over 6 feet per second. But the high current carried 280 million grains per second, because 1,140,000 cubic feet of water were then passing per second, while the low current carried but 100 million grains per second, or but little more than one-third as much; because the volume of water was then only 375,000 cubic feet per second.

At Columbus, 320 grains per cubic foot were carried with the highest current, 8¼ feet per second, in June, while 608 grains were carried in August with a current of 2.57 feet per second.

But when we bring in the absent dynamic elements of *space* and *time*, and ascertain by them the total quantity of work really done by the current at Columbus, we find that the river carried 444 million grains per second with the high current, and only 180 millions with the low current, because its volume of discharge with the high current was nearly 1,400,000 cubic feet per second, and only 280,000 with the low current. Hence it is simply impossible that the high water burden can be carried with the low rate of velocity without deposition occurring.

We learn from the illustration of the lever and weights, that the same force can only raise half the weight if it raise it to double the height in the same time. Hence we should not expect to find as much sediment *per cubic foot* in deep water, with a given velocity, as in shoal water. This fact will account for the quantity being greater per cubic foot in some of the measurements when the current was moderate, than when it was most rapid. The greater distance between the sediment and velocity lines during the first four months on diagram No. 2 is very marked. These were the high water months and the modifying effect of the depth of the stream on its power to suspend the sediment is clearly shown by the greater distance between these lines.

The depths as well as the velocities are usually greatest during floods. When the current was 8.25 feet per second, the depth at Columbus was 27 feet greater than when it was 2,57 feet per second, yet the tables show that the low current supported a greater quantity *per cubic*

foot than the higher velocity, because, *first*, it did not raise it so high above the bottom; and, *second*, because the river was falling. As many hours are necessary, even in still water, for all the sediment to fall, it must be evident that when the river is falling and the current diminishing, the water will have a greater amount in suspension than is then due to the velocity; and that when it is rising and the current increasing, it will then have less in suspension than the velocity would indicate. Therefore, the quantity found at a low velocity, if the river be falling rapidly, may be much greater *per cubic foot* of water, not only because of less depth, but also because of a diminishing velocity. The diagram (No. 2) shows that both causes operated to induce this great charge of 608 grains per cubic foot with this low rate of current.

The tables of sediment show also that the lower part of the water is somewhat more largely charged with sediment than the upper. This would act as an additional cause for the low water currents showing a larger ratio of sediment, particularly when the river has been falling some time. When it first begins to lose its high velocity, the largest particles, such as gravel, (which is undoubtedly carried in *suspension* with the higher velocities, in moderate depths) and coarse sand are first deposited. These fall rapidly, while the smaller particles require more time for settlement, according to their magnitudes and specific gravities. Fine particles of sand, which require the microscope to make them visible remain a long time suspended, and are carried with very low velocities. The material which forms blue and other clays is deposited during periods of low water and sluggish currents, and microscopic sand is always present in these alluvions. Many strata of hard blue clay were encountered by the piers of the St. Louis Bridge, when sinking them through the 80 feet of deposit overlying the limestone bed of the river. None of these were more than six or eight inches thick, and each was, no doubt, deposited during a single period of low water. They were alternated with layers of sand and gravel.

Caving banks generally occur when the river is falling, because then the support or pressure of the river having been withdrawn from them, such as have been undermined by the rapid highwater currents topple over into the stream and thus add temporarily to the normal charge of sediment then carried in suspension. It is quite possible that the high charge of 608 grains per cubic foot, with a velocity of only 2.57 feet per second, was partly due to caving banks a few miles above.

Diagram No 2 shows that in the eight months during which the sediment observations were made at Columbus, there were six periods when the river fell from levels previously attained, and at *each period* the quantity of suspended matter diminished *at once* with the loss of current. This instantaneous evidence of the intimate relation between the velocity and the quantity carried, so clearly shown by the *weekly* mean of these quantities on the diagram, would be less apparent in curves representing each experiment. Slight errors in weight, or in current measurements and local causes, such as the caving in of the banks above the observer, might make the sympathetic action between the current and sediment appear less harmonious if the mean of a number of experiments were not taken. The weekly mean taken by the authors of the report, thus tends to bring out in bolder light the force of their own testimony against them.

In addition to errors in measurement, and caving banks, other causes, such as the differently charged waters of tributaries moving with altered velocities in the parent stream, and the difference in the time required for different kinds of sediment to deposit, may each operate to modify the results of such experiments as these we are discussing, and hence absolute synchronism in the curves of velocity and sediment cannot be expected. This agreement is however, so marked in diagram No. 2, as to bear excellent testimony to the care with which Messrs. Webster and Fillebrown conducted the experiments at Columbus.

THE BED OF THE RIVER.

The wonderful discoveries made by Humphreys and Abbot, through their unique method of investigating dynamical phenomena, are supplemented with others in geology scarcely less sur-

prising. On page 14 of their Report we find the following:

"For instance, the Mississippi had always been regarded as flowing through a channel excavated in the alluvial soil, formed by the deposition of its own sedimentary matter. So important an assumption was inadmissible; and great pains were accordingly taken to collect specimens of the bed wherever soundings were made, and by every means to ascertain the depth of the alluvial soil from Cape Girardeau to the Gulf. This investigation has resulted in proving that the bed of the Mississippi is not formed in alluvial soil, but in a stiff, tenacious clay of an older geological formation than the alluvion."

The following occurs on page 91:

"What then constitutes the real bed of the river, upon which rest the moving sand-bars, and the new willow-batture formations? From the mouth of the Ohio down, at least as far as Ft. St. Philip [forty miles above the Gulf] it seems to be composed of a single substance, a hard, blue or drab-colored clay."

The age of the bed of the river is a matter of little practical interest to the public, and I do not therefore propose to discuss it. But whether it is composed of a clay that yields slowly to the strongest currents, and resists their action "almost like marble," is a question of the utmost importance to the people of the whole country. The intelligent reader need only be told that within three years, the Congress of the United States has been advised to incur an outlay of forty-six million dollars, based on the proposition that the bed of the Mississippi will not yield to the action of its strong current, to have his curiosity aroused upon this important question.

The existence of this substratum is asserted by Humphreys and Abbot in the most confident manner, *as a fact conclusively established by the numerous soundings of the Survey with prepared leads.* We are told on page 90, in reference to these soundings, that "The details of these operations are explained in Chapter IV, and the results exhibited in Appendix C."

Turning to Chapter IV, to learn by what devices this clay had been discovered "beneath the moving sand bars and the new willow batture formations," we find them to consist of nothing more than "a sounding chain and plummet." The latter is thus described: "The sinker, varying from ten to twenty lbs. in weight according to the force of the current, was a leaden bar whose bottom was hollowed out and armed with grease, in order to bring up specimens of the bed of the river; the patent lead was also used for the latter purpose."

Now, when it is remembered that *no borings were made either on the banks or in the bed of the river to test the existence of this unyielding clay*, the reader will appreciate how astonishingly the results of these soundings have been magnified, if he will examine them in Appendix C, and compare *the facts* there recorded with the extravagant reference made to them in the report.*

On page 90, under the heading of "*Geology of the channel*," we are told that "A knowledge of the character of the bed of the Mississippi River is of the highest practical importance, as will be hereafter seen, and great efforts have been made to acquire it."

The above extract, and the statement on page 14, that "great pains were accordingly taken to collect specimens of the bed wherever soundings were made," caused me to look forward to an examination of the results of these "great efforts," as a matter of considerable labor, more especially as they had been spoken of on page 412, as "an extended series of measurements." I carefully examined the first eleven tables of soundings in Appendix C, and found that they did really constitute "an extended series of measurements;" for they comprise the only recorded lines of soundings made by Humphreys and Abbot on the Mississippi River between Cape Girardeau and Vicksburg; a distance of 650 miles! The remaining tables are the record of soundings made at Vicksburg and below that point down to Fort St. Philip, a distance of 500 miles more.

As five of the eleven lines were run

* The record of the artesian well at New Orleans is given in the report, and reference is made to it on page 465 to prove that the river deposits overlying this ancient and imaginary clay, extends only 40 feet below the level of the gulf at New Orleans, (or 55 feet below high water mark,) As a sound cedar log was struck 153 feet deep by the auger, and is reported in the record, and therefore lies 98 feet deep in this marble like clay, it is to be regretted that an explanation of how it got there, was not given the report.

across the river at Columbus, and two at Lake Providence, the other four had necessarily to be considerably *extended* to make "this investigation" into the geology of 650 miles of river a very thorough one.

About fifty soundings, more or less, were made on each one of the eleven lines, but the grease was evidently bad, or the patent lead was a failure, for, on the first line of these numerous soundings, only *one* solitary sample was obtained. The grease seems to have given out altogether on four of the lines. When the two were run across at Lake Providence this must have been the case, or it was a bad day for geological research, because no specimen whatever was obtained in either of these two lines, and thus a space nearly two hundred miles long, between Napoleon and Vicksburgh—was not sampled at all. The prepared leads appear to have worked badly on the third line also, as only two samples were obtained there. In the entire eleven lines of soundings, that were made across the river in this 650 miles, there were only *thirty-five samples* of the bottom secured!

The different kinds of material were carefully noted in a separate column under the head of "Remarks."

When we reflect that each of these precious specimens was deemed to be a key to an unwritten record running away back into the dim past, where azoic and palæozoic cycles inclose the sublime genesis of the Father of Waters, we cannot fail to note the terse expressions with which, in such simple terms as "*Gravel*, *Clay*, *Sand*, or *Mud*," these antediluvian treasures are recorded. This brevity is however, fully compensated for in Chapter II, where "the results exhibited in Appendix C are discussed."

Let us now examine the conclusive evidence given of the existence of this unyielding substratum by "the samples of the bottom which were carefully preserved for examination and comparison."

The thirty-five samples secured in this 650 miles of river, when shorn of the imposing verbiage with which they are referred to in the report, certainly constitute a very small basis on which to rest the positive statement that the bed of the Mississippi is composed of an unyielding clay, even if we suppose each one of the samples was a specimen of *clay;* but this small basis becomes supremely ridiculous when the fact is stated, that twenty-five of these samples actually consisted of pure sand, and that only *seven* of the whole thirty-five were of clay alone! And then again, each one of the seven areas thus sampled by the prepared leads was probably not larger than the palm of a man's hand!

Moses, when stopped on Mount Pisgah, might as well have tried to analyze the subsoil of the promised land by gazing at it, afar off, as for these gentlemen to tell anything about a mythical substratum of clay under the shifting deposits of the river by means of their greased leads. The present age demands *proof*, not guesswork and assertion, and it is utterly impossible that anything adhering to the bottom of a tallowed plummet from the bed of the Mississippi, can furnish any evidence whatever as to the kind of material that lies one inch below where the sample was thus secured.

It is scarcely necessary to refer to the soundings below Vicksburg, after this statement, except to say that eighty-two lines were run in that part of the river, and that 56 of these were made in 45 miles of the river near New Orleans. In 116 miles of the river between Vicksburg and Natchez, only two samples were obtained. Of the total 93 lines run, no samples were obtained in 35 of them, and of all the samples taken, only about one in four was of clay alone, while more than one-half of the whole number were of pure sand. It is needless to say that all of the samples were just such materials as the river is constantly transporting in suspension, and that they do not furnish a particle of evidence that the bed is formed of any other substance than its own deposits.

Blue clay is one of the deposits or alluvions of the river, and is found everywhere in the alluvial basin, in layers alternating with the sand, gravel and earthy deposits, which compose its bed and banks. It is found deposited in old sunken wrecks,* on sunken rafts, and on the "rack heaps," or accumulations of drift-wood which lodge against snags

* Col. Andrews states that a barge which lay submerged during only two seasons of low water at the jetties had a stratum of blue clay nearly a foot thick deposited in it, which was so tough and sticky that the men could scarcely dig it out, because it adhered to the shovels so tenaciously.

or islands. It was doubtless an old steamboat wreck, or a rack heap which caused the loss of the sounding leads, referred to in Chapter II, and which marked the chain with this blue clay thirty feet above its broken end. Yet the clay, found on the chain and the uneven depths where it was broken, led the authors of the report to suppose that the river bottom was "full of blue clay ridges and lumps many feet high."

One proof of the fact that the bed of the river *does* yield readily to the action of the current will be seen in the great number of curved lakes lying on each side of its present bed, and extending from the upper to the lower end of the alluvial district. Each one of these was once a part of the river channel. The following correct explanation of their formation is copied from page 96 of the report:

"It occasionally happens that by this constant caving, two bends approach each other, until the river cuts the narrow neck of land between them and forms a 'cut-off,' which suddenly and materially reduces its length. The increased slope of the water surface at once makes this new bed the main channel of the river. The upper and lower mouths of the 'old river' are gradually silted up with sediment, drift-wood, etc., until eventually one of the crescent-shaped lakes so common in the alluvial region is formed."

The rapidity with which the current sometimes cuts away the tough blue clay, so frequently met with in its bed and banks, may be inferred from the following account of the formation of a cut-off, given by Major Suter, U. S. Engineers, in his report:

"Davis', one of the most recent of these cut-offs, and also the largest, occurred in 1867. It cut off Palmyra Bend, eighteen miles below Vicksburg, a bend which was eighteen miles long while the distance across the neck was only 1200 feet. The exact slope of the river at the time is not known, but it was probably not far from 0.3 foot to the mile; therefore the difference of level on the two sides of the neck was about 5½ feet. When the river broke through, the whole of the fall had to be absorbed in the 1200 feet of distance, making a rate of about twenty-four feet to the mile; and it can readily be imagined that the whole immense flood volume of the Mississippi, flowing with the enormous velocity due to this great slope, produced very marked effects. The roaring of the waters could be heard for miles; and in the course of a few hours, a channel a mile wide, certainly over a hundred and probably nearly two hundred feet in depth, had been excavated."

It is impossible to reconcile the excavation in a few hours of "a channel a mile wide and certainly over a hundred and probably two hundred feet deep," with the existence of a clay that "resists the action of the strong current, almost like marble." Such a clay is undoubtedly a myth.

THE PRACTICAL IMPORTANCE OF THESE TWO QUESTIONS.

Let us now look at the immense practical importance of these two facts which are so stoutly and dogmatically denied by Humphreys and Abbot. If the quantity of suspended sediment *is* regulated by the current, and if the bed of the river *is* formed of its own sedimentary deposits, instead of this unyielding and marble like clay, then it is entirely practicable to lower its flood line or slope, and deepen its channel by simply constructing light willow or brush dams during low water on the shoals which are then dry, or nearly so, at the various wide places in the river where the bars always exist. These dams would cause the deposit of more sediment on the shoals, by checking the current, and would deepen the contracted channels that would remain by increasing the current in them. In this way (without undertaking to straighten the river, which would be supremely foolish, and impracticable), the high water channel would be brought to a comparative uniformity of width, by gradually encouraging, from year to year, the deposition of sediment over the wide expanses, and this uniformity of width would produce a uniformity of depth, which in turn would insure a uniformity of current, and this would practically stop the caving of the banks. A uniformity in the width of the high water channel would do more however than all this, for it would lower the

flood line and practically dispense with the use of levees in protecting against overflow, an area equal to the state of Indiana.

If Humphreys and Abbot's theories are sound, such an improvement of the river channel, and such abandonment of the levee system, is totally impracticable.

The following quotations show that these dangerous theories have been adopted by the United States Levee Commission, which recently recommended a system of levees below the mouth of the Ohio at an estimated cost of nearly $46,000,000. It says in its report,* page 8, [Ex. Doc. 127 H. R. 43d C. 2d Ses.] that "the assumption that the river water is always charged with sediment to its maximum supporting capacity * * * has been shown by three years of accurate daily observations, at Carrollton and Columbus, to be utterly unfounded. Indeed, it often happened that the amount of sedimentary matter per cubic foot of water was greater in low than in high stages of the river, and never was there ever any fixed relation between these quantities. In other words, Mississippi River water is undercharged with earthy matter, and therefore no reasonable reduction of its flood velocity by an outlet will produce a deposit in the bed below."

By reference to pages 135 and 137 it will be seen that this extract contains an astonishing exaggeration. Instead of *three years*, the current and sediment observations only occupied *eight months* at Columbus, and *one* year at Carrollton.

When we remember that the junior author of the report on the Mississippi river, was a prominent member of the Levee Commission, and that the senior author, as Chief of Engineers, warmly endorsed its report, it is difficult to reconcile this careless statement with the unusual scientific exactness which required four decimals to record their measurements of the current, (see page 244). In this case the reader is converted to a false theory by being gravely assured that it has been *demonstrated conclusively by three years* of daily accurate measurements at the upper and lower ends of the delta; and in the other case, he is captivated by the wonderful precision which tells him to the ten thousandth part of a foot, the varying distances which the flowing stream has traveled at different depths below the surface, in a second of time! As this statement is an inexcusable exaggeration, and as such exact determination of current velocities is utterly impossible by any known method of measurement, it follows that theories sustained by such testimony, cannot constitute advances in science.

On page 16, of the report of the Commission, we find the following: "It is asserted in the most confident manner that the river is flowing in a bed composed of its own deposit, with dimensions regulated in accordance with its own needs; and hence that the increased velocity resulting from the confinement of its flood-volume between levees will rapidly excavate its bed to a correspondingly greater depth."

"This reasoning, if true, would establish conditions singularly fortunate for the Levee system; but unluckily the wish has been father to the thought. Uncompromising facts show that the premises and conclusion are both erroneous for the lower Mississippi. Very numerous soundings, with leads adapted to bring up samples of the bottom, were made by the Mississippi Delta Survey throughout the whole region between Cairo and the Gulf. They showed conclusively that the *real bed*, upon which rests the shifting sand bars and mud banks made by local causes, is always found in a stratum of hard blue clay, quite unlike the present deposits of the river. It is similar to that forming the bed of the Atchafalaya at its efflux, and, as is well known, resists the action of the strong current almost like marble."*

The results of these soundings with prepared leads are not only unduly magnified in the above statements, but the reader is also misled by the assurance that they *conclusively proved* the existence of this marble-like clay.

On page 17 of its Report this state-

*This report was reviewed by me in the *Scientific American* supplement.

*It is assumed, that because the efflux of the Atchafalaya has not deepened under the action of the current, the clay bottom there will not wear and must be something different from the ordinary river deposits. A bottom of sand would remain just as permanent when the capacity of the efflux is adjusted to the volume of discharge. The cross section of the bed, whether of clay or sand, will inevitably increase or diminish with an increase or diminution of the volume.

ment is made: "If we guard against these crevasses by raising and strengthening our levees, an elevation of the high water mark proportional to the increased volume will be sure to occur."

"To contain a quart of water a vessel must have exactly the requisite number of cubic inches; and a like principle applies with equal force to water in motion."

This is quite a novel proposition. How a like principle can apply to water in motion, I am at a loss to discover. The number of cubic inches in a quart cup is a question of space or volume only. When the water is in motion, *force* and *time* enter into the problem, and they make an elevation of the high water mark exactly proportional to the increased volume, a simple impossibility, even if the bed of the stream should not deepen. That the height would increase with the volume, as in the case of a quart cup, is simply an absurdity. But when problems in dynamics are solved without considering the elements of *space* and *time*, and the profound mysteries of remote geologic epochs, are unlocked with a greased sounding lead, we need not be surprised to learn that the most important questions in river hydraulics may be illustrated and explained with a quart cup.

If the bed of the river cannot yield, and all the crevasses in the levees are closed, the sides of the quart cup—or the levees, must be built up ten or eleven feet higher than ever before, and, therefore, the Levee Commission recommends, and the Chief of Engineers earnestly endorses, a system of levees at an estimated cost of $46,000,000, and all because the bed of the river has been conclusively proved by "an extended series of measurements," to be of an unyielding material.

A few years ago the Chief of Engineers of the U. S. Army, being equally as well convinced that the steamboat smoke pipes were, like the bed of the river, unyielding in their nature, and that they were too high to pass under the bridge, which spans the Mississippi at St. Louis, accordingly recommended that a canal with a draw-bridge, through the bridge approach, to accommodate these unyielding smoke pipes, should be dug around the end of the bridge in the ancient geologic blue clay in Illinois, at a cost of over three million dollars! The fact that the river water was proved by "a glance at the two diagrams" to be always under-charged with sediment, was an assurance that the canal would be a success and would not silt up. But Congress did not look with favor on this plan. Doubts as to the unyielding nature of the smoke pipes were openly expressed, and while the canal plans and estimates were being prepared the lucky discovery was made that the whole difficulty could be avoided by putting hinges in the pipes; and so the three million of public treasure was saved, and the commerce of the river now flows under the bridge without let or hindrance.

PRACTICABILITY OF DEEPENING THE RIVER AND LOWERING THE FLOODS.

The inclined plane formed by the *surface* of the river from the highlands down to the sea is called its slope. The intensity or *degree* of force exerted by the water in its passage depends upon the steepness of this slope. The *amount* of the force depends upon the mass or volume of the water and upon its velocity, the current being the result of the slope. The friction of the bed is the chief element which retards the current. The slope, the volume, and the friction are therefore the chief agents which determine the speed of the current. Others modify it somewhat but they need not be considered here.

Now if the reader will bear in mind that the water is charged with sediment according to its velocity, and that it flows through a bed of precisely the same kind of material it is carrying in suspension, and that if its velocity is increased it will take up a greater charge from its own bed, or if its current be slackened it will drop some of its charge in the channel, and add to its bed, he will understand the important part which the *speed* of the current performs in the problem. Through the whole alluvial basin from Cairo to the sea, the river must discharge as much sediment into the sea and over its banks, as its tributaries pour into it. If it discharged less, its channel would shoal up and its slope be steepened by the excess received from its tributaries.

If it carries more to the sea than is brought down into it from the tributaries, the excess discharged must be taken out of its own channel, and this would deepen it, and lower the slope. From this it is evident that there must be some means by which nature adjusts the speed of the current to suit the needs of the river. This is done by the relation which exists between the rate of current and the quantity of sediment carried in the water. If the velocity be too great the deepening of the bed follows. This lowers the slope and the current becomes less rapid. If the velocity on the contrary be too slow, deposition in the channel continues to take place until the river bottom is raised and the slope steepened, and a higher velocity is produced. These are the inexorable results of the relation between the current and its burden.

The river's slope, being the surface of the water, determines the height of the levees, and is therefore the vital question in the reclamation of the lands from overflow.

We see how the current alters the slope by the opposite processes of deposit and scour. We want to lower the slope to prevent overflow. When the current is too rapid, deepening is the process nature sets up in the bottom of the river, and gradually the slope is reduced and a normal current succeeds. To reduce the slope, we must temporarily increase the current. This can be done in two ways. Friction of the bed is the element which retards the velocity. Where the river is excessively wide, it will have more frictional resistance to overcome, and must there have a steeper slope. If we reduce its width at such place, the first effect will be an elevation of surface above. This will create a rapid current through the narrowed part, and it will be deepened there, and the elevation of surface above will then subside; but the current will still continue to be rapid, because the narrow and deep form of channel created will have less friction than the former wide one, and the rapid current will therefore continue to deepen the bed, until the original slope is so lowered that the current through the contracted channel is gradually reduced to the normal rate again. When this is done it will be found that the flood line or slope has been permanently lowered at that locality. This necessarily leaves the slope steeper immediately above the locality thus treated, and this induces a more rapid current, and consequent deepening of the bed, and lowering of slope still higher up. In this way the alteration of slope at one locality ultimately extends up to the head of the alluvial district. Of course this could not occur unless the most sensitive relation existed between the rate of current and the quantity of sediment suspended by it. Nor could it occur except where the bed of the river is formed of the same materials which it carries in suspension, or of materials easily eroded or moved by the current.

Another way to lower the slope is to increase the volume of water in the channel, because friction does not increase in an equal ratio with the volume. The greater is the volume, the lower is the slope, is a lesson taught by every part of the river, and by every outlet and bayou in the alluvial basin. This is because the proportion of friction to volume becomes less as the volume is increased, and, therefore, if the volume is increased, a lower slope will produce the normal rate of current, or that rate which will carry its charge of sediment to the sea without either loss or gain. It is impossible to maintain *permanently* any greater rate of current than will suffice to do this, in any sediment-bearing river in the world through its alluvial district. Bayou Atchafalaya at Red river carries a portion of the Mississippi to the sea with a fall of over six inches per mile, while the main river pursues a pathway more than three times as long, with a fall of less than two inches per mile. The greater friction in the smaller channel alone prevents a high rate of current through it. Its slope has been adjusted to maintain the rate required to discharge its waters and their earthy burden without injury to its own channel. If it were closed and its waters were compelled to flow in the main river, the first result would be an elevation of the surface and a more rapid current; a deepening of the bed would follow this, and a lowering of the slope would be the permanent result.

Lower levees would, of course, then be practicable. This teaches us that if we

wish to lower the floods and deepen the channel we must close the outlets and crevasses, and convey all of its waters through one channel to the sea. Humphreys and Abbot tell us precisely the contrary.

After an elaborate discussion on the effect of outlets and crevasses, they say: (page 420) "The conclusion is then inevitable, that *so far as the river itself is concerned they are of great utility.*"

The Levee Commission's report contains a table (page 59) from which it will be seen that from Cairo to Memphis (235 miles), there are 70 miles of crevasses and gaps in the levees, while many more exist below Memphis. It is well known that since the Rebellion in 1861, these levees have been going to destruction.

Certainly a sufficient number of outlets and crevasses have been existing and occurring here in the last 17 years to test their utility and the value of the opinion of these gentlemen on the subject.

Major Suter, U. S. Engineers, has made the most recent survey of the river, and in his report, 1875 (Ex. Doc. 19, Page 16, 43d Congress) he says: "Within the memory of living pilots the shoal water has extended down from Plum Point, one hundred miles above Memphis, to Lake Providence, fifty miles above Vicksburg, a total distance of 450 miles; and as these disturbing causes will act with more vigor every year, it is time that we should fairly face and realize the fact that, unless speedily checked, there are natural causes at work which will eventually destroy the navigability of the Mississippi and its tributary streams." Comment is unnecessary.

Since 1842, two large outlets have occurred, from artificial causes, through the narrow strip which separates the river from the gulf a few miles above the head of the passes. Through these about one-fifth of the river is now discharged. They are known as Cubitt's gap and The Jump. Surveys made in 1875 when compared with that of Talcot's made before they occurred, have revealed the fact that the depth of the river below the lowest one, has been reduced from over forty to thirty feet, and *the size of the river bed is fully one-quarter less* than it was before these crevasses occurred. I called public attention to this startling fact, to show that crevasses *do* cause shoaling in the river channel. Here is the explanation for this deposit, given by Genl. Humphreys. (See Appendix L, H. and A.'s report, 1876.) "During the low water stage of the river, there is a stratum of salt water many feet thick at the bottom in the passes and in the wide part of the river at the head of the passes, and extending above that point some distance, which has but little current either way compared to the current of fresh water on top of it; the earthy matter suspended in the river water falls upon the bottom of the river thus occupied by salt water, just exactly as it falls upon the bottom of the gulf out at sea beyond the bars, and during the low water stage a deposit is thus made on the bottom of the river."

On page 420 we are told that "there is no evidence that any filling up of the bed ever did occur in consequence of a high water outlet; and, moreover, that *it is impossible that it ever should occur*, either from the deposition of sedimentary matter held in suspension, or from the accumulation of material drifting along the bottom."

In view of the stubborn fact that this enormous shoaling *has occurred* since Cubitt's crevasse was made, it is plain that the above positive statement must be taken *cum grano salis.* Indeed it seems important for the credit of its authors that it be taken with a very large quantity of salt; for it appears that if there is a stratum of salt water under the river water, a shoal *will* occur below a crevasse. The feeblest current then, according to Gen'l Humphreys is not, *with salt under it*, capable of carrying so much sediment as the most rapid current; and the distribution of the sediment appears to be controlled by law if it has brine below it. The river water then ceases to be "always undercharged," and the relation between cause and effect is restored. The virtue of salt water is truly marvelous. "Old assumptions which experimental investigation has long since shown to be utterly unfounded in fact," become demonstrated truths, if a stratum of it be under the river water.

On page 415 of the report of Humphreys and Abbot, the following quotation is made from an article published by Major (now General) J. G. Barnard,

U. S. Engineers, in *Debow's Review* in 1850.

"'I find this principal laid down in the work of Frisi, 'On Rivers and Torrents,' which was placed in my hands by W. S. Campbell. He quotes and confirms the rules established by another engineer, Guglielmini, which are that 'the greater the quantity of water a river carries, *the less will be its fall*,' and 'the greater the force of the stream, the less will be the slope of its bed.' And, again, 'the slope of the bottom in rivers will diminish in the same proportion in which the body of water is increased,' and *vice versa*. These rules have their explanation in the facts that the beds of rivers, of the character above mentioned [like the lower Mississippi], are capable of resisting, unchanged, only a certain velocity of current; and, on the other hand, that the sedimentary matter contained in the river water, requires a certain degree of velocity to keep it in suspension. From the counteracting tendencies of the above two causes, a mean becomes established, at which the current ceases to deposit its sediment, and the bottom ceases to be abraded; in other words, the bottom becomes permanent. But if, from any cause, such as throwing off a portion of the water through a waste-weir, the velocity of the current is diminished, it is no longer able to maintain its sediment in suspension, but will continue to deposit in its bed, until, through the elevation of the bed, its velocity again becomes what it was before it was disturbed, sufficient to maintain its sediment in permanent suspension.'"

As this proposition is fully sustained by the Columbus and Carrollton experiments, and is conclusively proved by the phenomena presented all through the alluvial basin, the summary manner in which it is disposed of by Humphreys and Abbot is amusing. They say:

"It will be noticed that two important assumptions are necessary to support this reasoning: First, *that the bottom of the Mississippi is composed of its own alluvion, which can be readily acted upon by the current;* and, second, that its water is *always charged with sediment to the maximum capacity allowed by its velocity.*

"Throughout the whole distance from Cairo to Fort St. Philip the true bed consists of a tenacious clay which is unlike the alluvial soil, wears slowly under the strongest currents, and is, proved, by conclusive evidence, to belong to a geological formation antecedent to the present. This disposes of the first assumption.

"We come, then, to the second assumption, viz: that the water is at all times charged with sediment to the maximum capacity allowed by its velocity. * * * A glance at the two diagrams (plates XII and XIII) is sufficient to demonstrate the falsity of the assumption, that Mississippi water is always charged with sediment to the maximum capacity allowed by its velocity. * * * The second assumption is, then, as untenable as the first."

THE RELATION BETWEEN THE CURRENT AND SEDIMENT IS EXCEEDINGLY SENSITIVE.

Owing to the great width of the river at the head of the passes, the depth at the entrance into each pass is much shoaler than it is in the pass. South pass is about 700 feet wide, and over thirty feet deep, but the water entering it was about 2,800 feet wide half a mile above its entrance, and at this place the channel was but fourteen feet deep. To concentrate this 2,800 feet into a narrow and deep channel, I erected, with other more substantial works, a dam or willow screen 1,900 feet long across the current on the eastern side of this shoal. The dam consisted of a single thickness of willow mattress held in a vertical position by piles, the willow work being only two feet thick, and the depth of water being from twelve to sixteen feet. Of course the current passed through the willows with but little hindrance. It was not intended to be an impervious dam, and the whole structure was only strong enough to resist stormy weather. It was built with the practical knowledge that a *very slight* retardation of the current *will* cause a deposit. Two floods caused so great a deposit both above and below it that a small row boat can not now get to the dam at low tide. In another season or two, vegetation will probably cover this deposit and extend many hundred feet above the dam,

and an area of more than one hundred acres of dry land will occupy the space between the dam and the main land below. The channel through the shoal is now twenty-two feet deep at low tide.

In the Department of Public Works at St. Petersburg I was shown a device similar to a Venetian blind, formed with small ropes and wooden slats, that was said to have been successfully used on the Volga for the same purpose as the willow dam I have described.

These results can be explained on no other theory than that the amount of sediment carried is strictly regulated by the velocity of the current. The burden can only be carried by the expenditure of force. Nature adjusts the quantity to the force, and if we absorb any portion of the force even by the resistance of a porous willow dam, less force will remain to carry the burden and some of it must then fall to the bottom.

It is simply impossible that the work done, or load carried, can be greater than the force expended, or that the effect can be greater than the cause; and hence we cannot compel the force that is required by nature to transport the sediment, to do any other work, even so much as the turning of a mill wheel, or absorb any part of it by the friction of a dam made with open willow twigs, or even with one made with a fish net, without lessening, by so much, the force which is being expended in transporting the sediment. If we do, a deposition of a portion of the load must result, and it must continue to fall until, by the raising of the bed, a new regimen is established.

MOMENTUM AND VIS VIVA.

By S. BARNETT, Jr.

Written for Van Nostrand's Magazine.

In the June number of Van Nostrand's Engineering Magazine, we find the following from Prof. Skinner: ". . . . , I was arguing that writers who prefer to derive the unit of mass by definition from the unit of force ought to first make their arbitrary unit of force invariable, so that there should be a definite ratio between the units of mass and of force in the two systems; and so that students could pass by simple multiplication or division from one to the other."

Now, not to dwell upon the fact that some unit of mass or other must be determined before we can fix a unit of force, we may inquire what would be the nature of this ratio of the unit of mass to that of force. If the quotient of mass divided by force is an arithmetical number, that is of zero dimensions, mass and force are the same thing. Force would be nothing but mass, or mass nothing but force. But if mass is not force, the ratio of the two must be of dimensions other than zero in, at least, one denomination; say length or time, and this ratio will depend upon such other unit or units. It is necessary to show how the ratio so depends, and this Professor Tait showed in his Glasgow lecture in comparing force and momentum, or, at least, partly showed, but which Prof. Skinner said seemed to him "an arrangement of no validity."

Further, Prof. Skinner says: "But if *force* is nothing but a *rate of doing work*, then work is nothing but the action of a *rate of doing work*, and we may just as well say that *force* is *force* and *work* is *work*, and confess that we know nothing of either of them." We should hardly accept this, however, as the conclusion of the whole matter. There is not the least difficulty in the conception and exact expression of the product of the space passed over into the rate of change of momentum. In mathematical symbols

$$\text{work} = \int_b^a \frac{d(mv)}{dt} ds.$$

Also the rate of doing work per unit of length is force or the rate of change of momentum.

$$i.e., \quad \frac{d\,W}{ds} = \frac{d}{ds}\int_b^a \frac{d(mv)}{dt} ds = \frac{d(mv)}{dt}$$

The non-mathematical reader should

know that work is the sum of the elementary spaces passed through, each multiplied by the rate of change of momentum per unit of time at the point.

As regards the remarks of Thomson and Tait that, "It is therefore very much simpler and better to take the imperial pound" for the unit of mass, &c., we simply add—"Unquestionably so, for all practical purposes." And indeed the absolute unit of force only needs an absolute unit of mass no matter how derived; the assumption is only necessary so far as the unit of force is concerned. The practical difficulty of replacing units of mass by their relations to those of time and length has nothing to do with the theoretical perfection of the method.

REMARKABLE CHANGES IN THE EARTH'S MAGNETISM.*

From "Nature."

One of the most important, scientifically, of the special lectures at the Geographical Society, was that by Capt. Evans, in March last, on the subject of terrestrial magnetism. The concluding portion, especially, is of high scientific importance. Capt. Evans gave a historical sketch of the subject of terrestrial magnetism from the time of the discovery of the dip of the magnetic needle. After speaking further on various departments of his subject, Capt. Evans went on to say:

We have now passed in review the successive stages of development of our branch of knowledge, from the pregnant epoch when its principles were enunciated by Gilbert, till the period when the well-directed munificence of his own and other Governments dotted the earth's surface with observatories, and despatched land and sea expeditions, specially equipped, for the determination of the magnetic elements. We have seen how a few earnest and gifted men have, by long and patient analysis, laid the foundations for future generations to build upon as regards theory, and unravelled the apparently inextricable web surrounding the needle's daily and yearly movements; tracing these movements to their primary source, the sun: and how by the perseverance of states and of individuals, we are now in possession of accurate knowledge as to the distribution of magnetism over the surface of our globe, as represented by the variation and dip of the needle, and by the measure of the force connected with those component elements. But the task, from a scientific point of view, is far from completed while we remain in ignorance of the causes of greater changes in the earth's magnetism going on from year to year, and so on, possibly through æons of time. From a practical point of view, so far as the interests of men are concerned, the collection of records will be a never ending task, for every generation must observe and chart the magnetic elements of its time.

The subject of secular change is thus one of such great interest that the remaining portion of my lecture must be chiefly devoted to it. The active mind of Halley was drawn, as one of the first, to the probable nature of the causes; collecting such observations of the variation of the compass as had then been made, and projecting them on polar maps, he found that the convergence of the several directions of the needle led to two points in each hemisphere. On this he enunciated the proposition "that the whole globe of the earth is one great magnet, having four magnetical poles or points of attraction; near each pole of the equator two; and that in those parts of the world which lie near adjacent to any of these magnetic poles the needle is governed thereby, the nearest pole always being predominant over the more remote." Halley saw, as he confessed with despair, the difficulties attending the proposition, "as never having heard of a magnet having four poles," but there were the facts manifested by the earth, and he was too sagacious and sound a

* From Lecture at the Royal Geographical Society, March 11, by Captain F. J. Evans, C.B., F.R.S, Hydrographer to the Admiralty.

philosopher to pass them by. He accordingly propounded a theory which, however fantastic it may now appear, and perhaps did at the time he wrote, has nevertheless within it the fire of genius, and may probably be found yet to contain some sparks of truth. To account for the four poles, and at the same time for the secular change of the variation, he conceived that the earth itself might be a shell, containing within a solid globe, or terella, which rotated independently of the external shell; each globe having its own magnetic axis passing through the common center; but the two axes inclined to each other and to that of the earth's diurnal rotation. It is not difficult to follow the movements of the consequent four imaginary poles in solution of the problem.

Hansteen working at the same problem a century after Halley [1811–19], and much on the same lines, came nearly to the same conclusion with regard to the four poles of attraction; and he rendered justice to Halley by recognizing him as the first who had discovered the true magnetic attraction of the globe. Hansteen, with the material at his command, went however a step further, and computed both the geographical positions and the probable period of the revolution of this dual system of poles or points of attraction round the terrestrial pole. From these computations he found that the North American point or pole required 1,740 years to complete its grand circle round the terrestrial pole, the Siberian 860 years; the pole in the Antarctic regions south of Australia, 4,609 years; and a secondary pole near Cape Horn, 1,304 years. The influence of these laborious investigations on the minds of subsequent inquirers may easily be imagined.

The matured views of Sir Edward Sabine on the secular changes—enunciated in the clearest manner in 1864–72—are deserving of the highest consideration. An ardent admirer of the genius and no less of the sagacity of Halley, he in part follows Halley's views, and considers that two magnetic systems are directly recognisable in the phenomena of the magnetism of the globe; the one having a terrestrial, the other a cosmical origin. The magnetism *proper* of the globe, with its point of greatest attraction (*i.e.* in the northern hemisphere) in the north of the American continent is the stronger; the weaker system, or that which results from the magnetism induced in the earth by *cosmical action*, with its point of greatest attraction is, at present, in the north of the Asiatic continent. Sir Edward Sabine also expresses his belief that "it is the latter of these two systems which by its progressive translation, gives rise to the phenomena of secular change, and to those magnetical cycles which owe their origin to the operation of the secular change."

Reviewing these several hypotheses by the light of observations made in recent years, it is difficult, and indeed in some directions, impossible to recognise their accordance with changes now going on; there can be no doubt, notwithstanding, that Halley and Hansteen analyzed their facts with skill, and that their deductions were borne out by those facts. In explanation of this anomaly it is necessary to glance retrospectively on the changes in progress at the times in which these philosophers gave utterance to their views [1700–1819]. During this long interval, and, so far as relates to parts of the northern hemisphere, for a century before, there was in the higher latitudes a general movement of the north end of the needle in the following directions:

Over all that area (embracing the Atlantic and Indian Oceans) from Hudson's Bay to about the meridian of the North Cape of Europe, and from Cape Horn to about the western part of Australia, the north end of the needle was successively drawn to the west at a maximum rate of 8′ or 10′ a year. From the meridian of the North Cape of Europe to that of 130° east, it was successively drawn to the east, while from thence to Hudson's Bay it was nearly stationary, or perhaps oscillated a little; in the southern hemisphere, from about the western part of Australia to Cape Horn, the movement was throughout to the east at the maximum rate of 7′ a year. There was thus a general uniformity of movement; in that hemisphere (dividing the globe into *eastern* and *western* hemispheres) which includes the Atlantic and Indian Oceans, the needle was constantly drawn more and more to the west; in the hemisphere

embracing the Pacific Ocean, more and more to the east.

So far then to the early part of the present century we can trace a harmonious movement of the needle over the whole globe, justifying the conclusions of our old philosophers; but in the year 1818 at London, and generally contemporaneous with that epoch throughout Europe and North Africa, the westerly progress of the north end of the needle ceased, and an easterly movement commenced; this continues to the present time, and with a yearly increasing rate. But in the South Atlantic during this period the westerly movement has never ceased; it is still going on, and in some parts with rapidity. Here, then, is a marked dislocation of the harmonious regularity embodied in Halley's and Hansteen's calculations and conceptions.

The matured views of Sir Edward Sabine, to which I have drawn attention, seem to anticipate the difficulties attendant on this new and complex movement; for, if I apprehend his meaning correctly, they imply that the poles of attraction which have a terrestrial source, *i.e.* the *magnetic poles*, are not subject to translation.*

The hypothesis, if further followed, is nevertheless beset with difficulties; for we can scarcely conceive changes due to *cosmical* action to be otherwise than general in character, and to affect the whole globe. Thus, if the progressive translation of the induced or weaker system in Northern Asia—and presumably of that in the southern hemisphere—were the direct causes of the secular charges, we should anticipate uniformity in the general movements of the needle as manifested by its variation and dip over the earth's surface. But this is contrary to modern experience; for in some regions great activity of movement, both in the direction of pointing and in the inclination of the needle, is going on; in others there is comparative repose in both elements; while in another region the needle remains nearly constant in its direction, while its inclination sensibly varies from year to year. For example:

A region of remarkable activity presents itself in the South Atlantic Ocean; a great part of the seaboard of South America extending to Cape Horn, and including St. Paul's Rocks, Ascension, St. Helena, and the Falkland Islands, with their adjacent seas, are embraced therein. In some parts of this area the westerly movement of the needle exceeds 7′ or 8′ a year, and has so progressed for nearly three centuries. On the American coast the dip of the south end of the needle *decreases* from 7.5′ to 4′ yearly, while from the Cape of Good Hope to Ascension it *increases* from 5′ to 10′ yearly. We have here, within narrow limits, a noteworthy dislocation of the observed phenomena.

Another region of activity, so far as is denoted by the changes of variation, extends over Europe, Western Asia, and North Africa. Here the needle, in opposition to the protracted westerly movement going on in the South Atlantic, commenced moving to the eastward in the early part of this century; it has a progressive rate which in some parts now amounts to 10′ a year. The dip diminishes in this region seldom more than 3′ a year.

A region of activity, so far as the dip is concerned, but with little change in the variation, is to be found on the west coast of South America; at Valparaiso, as at the Falkland Islands, the south dip decreases at the rate of 7′ yearly, but in sailing northward and reaching the 10th degree of south latitude, this active movement appears to cease.

But little activity in either element now exists over the habitable part of the North American continent or in the West Indies. Throughout China there is little change in the variation, but an *increasing* dip of 3′ or 4′, and thus a reverse movement to that going on in Europe.

Over a great part of the Western Pacific Ocean, as also in Australia and New Zealand, there is so little change in the two elements that this may be termed a region of comparative repose.

These are a few facts relating to secular changes going on in two magnetic elements within our own time; and what are the inferences to be drawn therefrom? They appear to me to lead to the conclusion that movements, cer-

* So far as modern observations bear on the position of the magnetic poles, they indicate permanency rather than change of place.

tainly beyond our present conception, are going on in the interior of the earth; and that so far as the evidence presents itself, secular changes are due to these movements and not to external causes; we are thus led back to Halley's conception of an internal nucleus or inner globe, itself a magnet, rotating within the outer magnetised shell of the earth.

We need not here pause to discuss the probability of this fanciful conception of the old philosopher, but proceed to examine how far the behavior of another element, the intensity of the earth's magnetism, confirms the view that movements are going on in the interior of our globe. In common I believe with all those who have pursued the study of this element, from the time when Sabine's original memoir to the British Association (1837) threw so much light on this special division of the subject, I had conceived that stability, within very limited conditions, was a distinctive condition of the earth's force; and that it was alone by watchful attention to the instruments of precision devised for its determination that changes in short intervals of time, such as a generation, could be detected.* If we turn to the results obtained in this country through nearly half a century, it is possible that an *increase* of two or three hundredths of the total force may be found. In Italy at the present time the annual *decrease* has been given by that active observer, the Rev. Father Perry, as .004; so also on the North American continent, where, as we are told by the zealous magnetician, Schott, there is evidence of the force slightly increasing at Washington, of being stationary at Toronto, in Canada, and slightly decreasing at Key West, in the Gulf of Mexico. So far stability, within very small limits, obtains over a very large part of the northern hemisphere. If, however, we turn to the continent of South America and its adjacent seas (parts of which are regions of marked activity as denoted by changes in the variation and dip of the needle), we shall find a diminution of the intensity of the earth's force now going on in a remarkable degree; an examination of the recent observations made by the *Challenger's* officers at Valparaiso and Monte Video, compared with those made by preceding observers, show that within half a century the whole force had respectively diminished one-sixth and one-seventh—at the Falkland Islands one-ninth. Farther north we find at Bahia and Ascension Island, in the same period of time, an equally marked diminution of one-ninth of the force. This area of *diminishing* force has wide limits; it would appear to reach the equator and to approach Tahiti on the west and St. Helena on the east; at the Cape of Good Hope there is evidence of the force *increasing*.

Such are the facts, and how are we to interpret them? Whichever way we look at the subject of the earth's magnetism and its secular changes, we find marvelous complexity and mystery; lapse of time and increase of knowledge appear to have thrown us farther and farther back in the solution. The terella of Halley, the revolving poles of Hansteen, and the more recent hypotheses of the ablest men of the day, all fail to solve the mystery. We must not, however, be discouraged at these repulses in the great conflict for the advancement of human knowledge. The present century has been productive of keen explorers in the field of terrestrial magnetism; others emulous of fame are pressing rapidly from the rear, and knowing as we do that knowledge shall be increased, we may confidently anticipate the day when this, one of Nature's most formidable secrets, shall be revealed.

* The investigations of that able magnetician, Mr. Broun, led him to consider that the earth's magnetic force increases and diminishes from day to day by nearly the same amount over the whole globe. These increases and diminutions have been traced to the action of the sun in such a way that the greatest of them recur frequently at intervals of twenty-six days, or multiple of twenty-six days—a period attributable to the sun's rotation.

THE telephone has been adopted on the mountain section of the Central Pacific Railway. The points supplied are Truckee, Blue Cañon, Summit, Cascade, Strong's Cañon, Yuba Pass, Tamarack, and Camp 3. The main office is at Blue Cañon, and each track-walker is compelled to report himself both in passing east and west. The telephones are to be placed at distances of a very few miles apart, to enable the "track-walkers," or platelayers, to make any necessary requests or other communications, as to state of road.

THE THEORY OF INTERNAL STRESS IN GRAPHICAL STATICS.

By HENRY T. EDDY, C. E., Ph. D., University of Cincinnati.

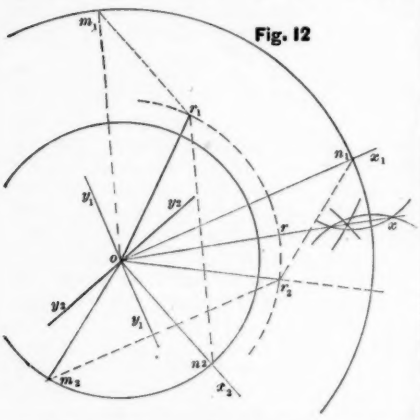
Written for VAN NOSTRAND'S MAGAZINE.

III.

COMBINATION AND SEPARATION OF STATES OF STRESS.

PROBLEM 19.—When two given states of right shearing stress act at the same point, and their principal stresses have a given inclination to each other, to combine these states of stress and find the resultant state.

In Fig. 12 let ox_1, ox_2 denote the directions of the two given principal + stresses, and let $a_1=on_1$, $a_2=on_2$ repre-



sent the position and magnitude of these principal stresses. Since the given stresses are right shearing stresses $a_1=-b_1$, $a_2=-b_2$ and the respective planes of shear bisect the angles between the principal stresses. Now it has been previously shown that the intensity of the stress caused by the principal stresses $a_1=-b_1$ is the same on every plane traversing o: the same is true of the principal stresses $a_2=-b_2$: hence, when combined, they together produce a stress of the same intensity on every plane traversing o. This resultant state of stress evidently does not cause a normal stress on every plane, hence the resultant state must be a right shearing stress.

Let us find its intensity as follows: The principal stresses $a_1=-b_1$ cause a stress on_1 on the plane y_1y_1, and the principal stresses $a_2=-b_2$ cause a stress om_2 on the same plane in such a direction that $x_2om_2=x_1ox_2$, as has been before shown. Complete the parallelogram $n_1om_2r_2$; then or_2 represents the intensity and direction of the stress on y_1y_1. But the principal stresses bisect the angles between the normal and the resultant intensity, therefore, ox, which bisects x_1or_2, is the direction of a principal stress of the resultant state, and $or=or_2=a$ is the intensity of the resultant stress on any plane through o.

The same result is obtained by finding the stress the plane y_2y_2, in which case we have $on_2=a_2$ acting normal to the plane, and $om_1=a_1$ in such a direction that $x_1om_1=x_2ox_1$. The sides and angles of $n_2om_1r_1$ and $n_1om_2r_2$ are evidently equal, hence the resultants are the same, $or_1=or_2=a$, and ox bisects x_2or_1.

The algebraic solution of the problem is expressed by the equation,

$$a^2=a_1^2+a_2^2+2a_1a_2\cos 2x_1x_2,$$

from which a may be found, and, finally, the position of or is found from the proportion,

$$\sin 2xx_1 : a_2 :: \sin 2xx_2 : a_1 :: \sin 2x_1x_2 : a.$$

PROBLEM 20.—When any two states of stress, defined by their principal stresses, act at the same point, and their principal stresses have a given inclination to each other, to combine these states and find the resultant state.

Let a_1, b_1, and a_2, b_2 be the given principal stresses, of which a_1 and a_2 have the same sign and are inclined at a known angle x_1x_2, but in so taking a_1 and a_2 they may not both be numerically greater than b_1 and b_2 respectively.

Separate the pair of principal stresses a_1b_1 into the fluid stress $+\frac{1}{2}(a_1+b_1)$, and the right shearing stress $\pm\frac{1}{2}(a_1-b_1)$ as

has been previously done; and in a similar manner the principal stresses a_2 b_2 into $+\frac{1}{2}(a_2+b_2)$ and $\pm\frac{1}{2}(a_2-b_2)$. Then the combined fluid stresses produce a fluid stress of $+\frac{1}{2}(a_1+b_1+a_2+b_2)$ on every plane through o; and the combined right shearing stresses cause a stress whose intensity and position can be found by Problem 19.

The total stress is obtained by combining the total fluid stress with the resultant right shearing stress.

Of course, any greater number of states of stress than two, can be combined by this problem by combining the resultant of two states with a third state and so on.

The algebraic expression of the combination of any two states of stress is as follows:

$$(a+b)=(a_1+b_1+a_2+b_2),$$

$$(a-b)^2=(a_1-b_1)^2+(a_2-b_2)^2+2(a_1-b_1)(a_2-b_2)\cos 2x_1x_2,$$

$$\therefore\ a=\tfrac{1}{2}(a_1+b_1+a_2+b_2+[(a_1-b_1)^2+(a_2-b_2)^2+2(a_1-b_1)(a_2-b_2)\cos 2x_1x_2]^{\frac{1}{2}}),$$

$$b=\tfrac{1}{2}(a_1+b_1+a_2+b_2-[(a_1-b_1)^2+(a_2-b_2)^2+2(a_1-b_1)(a_2-b_2)\cos 2x_1x_2]^{\frac{1}{2}}),$$

in which a and b are the resultant principal stresses. Also, $\sin 2xx_1 : a_2-b_2$ $:: \sin 2xx_2 : a_1-b_1 :: \sin 2x_1x_2 : a-b$.

PROBLEM 21.—In a state of stress defined by the stresses upon two planes at right angles to each other, to find the principal stresses.

Let the given stresses be resolved into tangential and normal components; it has been shown that the tangential components upon these planes are of equal intensity and unlike sign. Let the intensity of the tangential component be a_t, and that of the normal components a_n and b_n respectively. The tangential components together constitute a state of right shearing stress of which the given planes are the planes of shear, and the principal stresses bisect the angles between the given planes.

Separate the remaining state of stress into the fluid stress $+\frac{1}{2}(a_n+b_n)$ and the right shearing stress $\pm\frac{1}{2}(a_n-b_n)$, and combine this last right shearing stress with that due to the tangential components. The final result is found, just as in Problem 20, by combining the fluid stress $\frac{1}{2}(a_n+b_n)$ with the resulting right shearing stress.

This problem can also be solved in a manner similar to that employed in Problem 6.

The result is expressed by the equations,

$$a+b=a_n+b_n,$$

$$(a-b)^2=(a_n-b_n)^2+4a_t^2$$

for the angle which has been heretofore denoted by x_1x_2 is in this case $45°$ $\therefore \cos 2x_1x_2=0$

$$\therefore\ a=\tfrac{1}{2}(a_n+b_n+[(a_n-b_n)^2+4a_t^2]^{\frac{1}{2}})$$

$$b=\tfrac{1}{2}(a_n+b_n-[(a_n-b_n)^2+4a_t^2]^{\frac{1}{2}})$$

$\sin 2xx_1 : 2a_t :: \sin 2xx_2 : a_n-b_n$ $:: 1 : a-b$,

but $2xx_1=90°-2xx_2$,

$\therefore \tan 2xx_1=2a_t \div (a_n-b_n)$.

PROBLEM 22.—In a state of stress defined by two simple stresses which act at the same point and have a given inclination to each other, to combine them and find the resultant state.

It has been previously mentioned that any simple stress as a_1 can be separated into the fluid stress $+\frac{1}{2}a_1$ and the right shearing stress $\pm\frac{1}{2}a_1$, as it is simply a case in which $b_1=0$. Hence the simple stresses a_1, a_2 can be combined as a special case of Problem 20, in which b_1 and b_2 vanish. The results are expressed algebraically as follows:

$$a+b=a_1+a_2,$$

$$(a-b)^2=a_1^2+a_2^2+2a_1a_2\cos 2x_1x_2,$$

$$\therefore\ ab=\tfrac{1}{2}a_1a_2(1-\cos 2x_1x_2)$$

$$\therefore\ ab=a_1a_2\sin^2x_1x_2.$$

Since a simple compression or tension produces a simple stress in material, this problem is one of frequent occurrence, for it treats the superposition of two, and hence of any number of simple stresses lying in the same plane.

This problem is of such importance that we think it useful to call attention to another solution of it, suggested by the algebraic expressions just found.

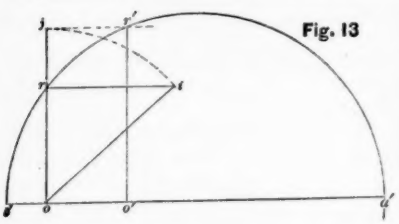
In Fig. 13 let

$$o'a'=a_1,\ o'b'=a_2\ \therefore\ o'r'=\sqrt{a_1a_2}=oi.$$

Now, if $oir=x_1x_2$, then $or=o'r'\sin x_1x_2$

$$\therefore\ \overline{or}^2=oa'.ob'=o'a'.o'b'\sin^2x_1x_2$$

$$\therefore\ oa'=a \text{ and } ob'=b.$$



This solution is treated more fully in Problem 23.

PROBLEM 23.—When a state of stress is defined by its principal stresses, it is required to separate it into two simple stresses having a given inclination to each other.

It was shown in Problem 22 that $a+b=a_1+a_2$, and $ab=a_1a_2 \sin x_1x_2$.

Let us apply these equations in Fig. 13 to effect the required construction. Make $oa'=a$, $ob'=b$; then $a'b'=a_1+a_2$. At o erect a perpendicular to $a'b'$ cutting the circle of which $a'b'$ is the diameter at r; then $\overline{or}^2=ab$, the product of the principal stresses. Also make $a'oi=x_1x_2$ the given inclination of the simple stresses, and let $ri \parallel a'b'$ intersect oi at i; then $or=oi \sin x_1x_2 \therefore \overline{oi}^2=a_1a_2$.

Make $oj=oi$ and draw $jr' \parallel a'b'$, then

$$o'r'=oi, \text{ and } o'a'.o'b'=\overline{o'r'}^2,$$
$$\therefore o'a'=a_1 \text{ and } o'b'=a_2,$$

the required simple stresses. This construction applies equally whether the given principal stresses are of like or unlike sign, and also equally whether the two simple stresses are required to have like or unlike signs.

PROBLEM 24.—When a state of stress is defined by its principal stresses, to find the inclination of two given simple stresses into which it can be separated.

In Fig. 13 let $oa'=a$, $ob'=b$ be the intensities of the principal stresses, and $o'a'=a_1$, $o'b'=a_2$ be the intensities of the given simple stresses. It has been already shown that $a+b=a_1+a_2$. Draw the two perpendiculars or and $o'r'$; through r draw $ri \parallel a'b'$; make $oi=oj=o'r'$; then is $oir=ioa'$ the required inclination, for it is such that

$$ab=a_1a_2 \sin^2 x_1x_2$$

PROBLEM 25.—To separate a state of right shearing stress of given intensity into two component states of right shearing stress whose intensities are given, and to find the mutual inclination of the principal stresses of the component states.

In Fig. 12, about the center o, describe circles with radii $on_1=a_1$, $on_2=a_2$, the given component intensities; and also about o at a distance $or_1=a$, the given intensity. Also describe circles with radii $r_1m_1=on_2$, $r_1n_2=on_1$ cutting the first mentioned circles at m_1 and n_2: then is $\frac{1}{2}n_2om_1=x_1x_2$ the required mutual inclination of the principal stresses of the component states. This is evident from considerations previously adduced in connection with this figure. The relative position of the principal stresses and principal component stresses is also readily found from the figure.

PROBLEM 26.—In a state of right shearing stress of given intensity to separate it into two component states of right shearing stress, when the intensity of one of these components is given and also the mutual inclination of the principal stresses of the component states.

In Fig. 12, about the center o describe a circle rr with radius $or=a$, the intensity of the given right shearing stress, and at n_1, at a distance $on_1=a_1$ from o which is the intensity of the given component, make $x_1n_1r_2=2x_1x_2$, twice the given mutual inclination; then is n_1r_2 the distance from n_1 to the circle rr the intensity of the required component stress. The figure can be completed as was done previously.

It is evident, when the component a_1 exceed a, that there is a certain maximum value of the double inclination, which can be obtained by drawing n_1r_2 tangent to the circle rr, and the given inclination is subject to this restriction.

Other problems concerning the combination and separation of states of stress can be readily solved by methods like those already employed, for such problems can be made to depend on the combination and separation of the fluid stresses and right shearing stresses into which every state of stress can be separated.

PROPERTIES OF SOLID STRESS.

We shall call that state of stress at a point *a solid stress* which causes a stress on every plane traversing the point. In the foregoing discussion of plane stress no mention was made of a stress on the plane of the paper, to which the plane stress was assumed to be parallel. It is, evidently, possible to combine a simple stress perpendicular to the plane of the paper with any of the states of stress heretofore treated without changing the stress on any plane perpendicular to the paper.

Hence in treating plane stress we have already treated those cases of solid stress which are produced by a plane stress combined with any stress perpendicular to its plane, acting on planes also perpendicular to the plane of the paper.

We now wish to treat solid stress in a somewhat more general manner, but as most practical cases are included in plane stress, and the difficulties in the treatment of solid stress are much greater than those of plane stress, we shall make a much less extensive investigation of its properties.

CONJUGATE STRESSES.—Let xx, yy, zz be any three lines through o; now, if any state of stress whatever exists at o, and xx be the direction of the stress on the plane yoz, and yy that on zox, then is zz the direction of the stress on xoy: *i.e.*, each of these three stresses lies in the intersection of the planes of action of the other two.

Reasoning like that employed in connection with Fig. 1, shows that no other direction than that stated could cause internal equilibrium; but a state of stress is a state of equilibrium, hence follows the truth of the above statement.

TANGENTIAL COMPONENTS.—Let xx, yy, zz be rectangular axes through o; then, whatever may be the state of stress at o, the tangential components along xx and yy are equal, as also are those along yy and zz, as well as those along zz and xx.

The truth of this statement flows at once from the proof given in connection with Fig. 3.

It should be noticed that the total shear on any plane xoy, for example, is the resultant of the two tangential components which are along xx and yy respectively.

STATE OF STRESS.—Any state of solid stress at o is completely defined, so that the intensity and direction of the stress on any plane traversing o can be completely determined, when the stresses on any three planes traversing o are given in magnitude and direction.

This truth appears by reasoning similar to that employed with Fig. 4, for the three given planes with the fourth enclose a tetrahedron, and the total distributed force acting against the fourth plane is in equilibrium with the resultant of the forces acting on the first three.

PRINCIPAL STRESSES.—In any state of solid stress there is one set of three conjugate stresses at right angles to each other, *i.e.* there are three planes at right angles on which the stresses are normal only.

Since the direction of the stress on any plane traversing a given point o can only change gradually, as the plane through o changes in direction, it is evident from the directions of the stresses on conjugate planes that there must be at least one plane through o on which the stress is normal to the plane. Take that plane as the plane of the paper; then, as proved in plane stresses, there are two more principal stresses lying in the plane of the paper, for the stress normal to the plane of the paper has no component on any plane also perpendicular to the paper.

FLUID STRESS.—Let the stresses on three rectangular planes through o be normal stresses of equal intensity and like sign; then the stress on any plane through o is also normal of the same intensity and same sign.

This is seen to be true when we combine with the stresses already acting in Fig. 5, another stress of the same intensity normal to the plane of the paper.

RIGHT SHEARING STRESS.—Let the stresses on three rectangular planes through o be normal stresses of equal

intensity, but one of them, say the one along xx, of sign unlike that of the other two; then the stress on any plane through o, whose normal is $x'x'$, is of the same intensity and lies in the plane xox' in such a direction rr that xx and the plane yz bisect the angles in the plane xox' between rr and its plane of action, and rox' respectively.

The stress parallel to yz is a plane fluid stress, and causes therefore a normal stress on the plane xox'. Hence the resultant stress is in the direction stated, as was proved in Fig. 6.

Component States of Stress.—Any state of solid stress, defined by its principal stresses abc along the rectanglar axes of xyz respectively, is equivalent to the combination of three fluid stresses, as follows:

$\frac{1}{2}(a+b)$ along x and y, $-\frac{1}{2}(a+b)$ along z;
$\frac{1}{2}(c+a)$ along z and x, $-\frac{1}{2}(c+a)$ along y;
$\frac{1}{2}(b+c)$ along y and z, $-\frac{1}{2}(b+c)$ along y;

For these together give rise to the following combination:

$\frac{1}{2}(a+b)+\frac{1}{2}(c+a)-\frac{1}{2}(b+c)=a$, along x;
$\frac{1}{2}(a+b)-\frac{1}{2}(c+a)+\frac{1}{2}(b+c)=b$, along y;
$\frac{1}{2}(a+b)+\frac{1}{2}(c+a)+\frac{1}{2}(b+c)=c$, along x.

In case $b=0$ and $c=0$ this is a simple stress along x.

Component Stresses.—Any state of solid stress defined by its principal stresses can also be separated into a fluid stress and three right shearing stresses, as follows:

$\frac{1}{2}(a+b+c)$ along x, y, z;
$\frac{1}{2}(a-b-c)$ along x, and $-\frac{1}{2}(a-b-c)$ along y and z;
$\frac{1}{2}(b-c-a)$ along y, and $-\frac{1}{2}(b-c-a)$ along z and x;
$\frac{1}{2}(c-a-b)$ along z, and $-\frac{1}{2}(c-a-b)$ along x and y;

It will be seen that the total stresses along $x\ y\ z$ are $a\ b\ c$ respectively. This system of component stresses is remarkable because it is strictly analagous in its geometric relationships to the trammel method used in plain stress. We shall simply state this relationship without proof, as we shall not use its properties in our construction.

If the distances $pa_1=a$, $pb_1=b$, $pc_1=c$ be laid off along a straight line from the point p, and then this straight be moved so that the points $a_1\ b_1\ c_1$ move respectively in the planes yz, zx, xy; then p will describe an ellipsoid, as is well known, whose principal semiaxes are along xyz, and are abc respectively. Now the distances pa_1, pb_1, pc_1, may be laid off in the same direction from p or in different directions; so that, in all, four different combinations can be made, either of which will describe the same ellipsoid. But the position of these four generating lines through any assumed point $x_1y_1z_1$ of the ellipsoid is such that their equations are

$$\frac{a}{x_1}(x-x_1)=\pm\frac{b}{y_1}(y-y_1)=\pm\frac{c}{z_1}(z-z_1)$$

Now if the fluid stress $\frac{1}{2}(a+b+c)=or_1$ be laid off along the normal to any plane, *i.e.* parallel to that generating line which in the above equation has all its signs positive, and the other three right shearing stresses r_1r_2, r_2r_3, r_3r_4 be laid off successively parallel to the other generating lines, as was done in plane stresses, the line or_4 will be the resultant stress on the plane.

Problems in Solid Stress.

Problem 27.—In any state of stress defined by the stresses on three rectangular planes, to find the stress on any given plane.

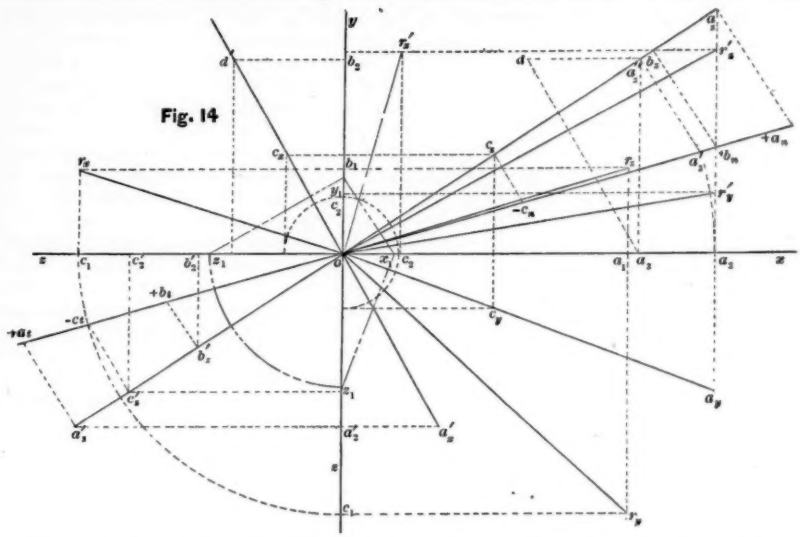
Let the intensities of the normal components along $x\ y\ z$ be $a_n\ b_n\ c_n$ respectively, and the intensities of the pairs of tangential components which lie in the planes which intersect in $x\ y\ z$ and are perpendicular to those axes be $a_t\ b_t\ c_t$ respectively, *e.g.*, a_t is the intensity of the tangential component on xoy along y, or its equal on xoz along z.

In Fig. 14 let a plane parallel to the given plane cut the axes at $x_1y_1z_1$; then the total forces on the area $x_1y_1z_1$ along xyz are respectively:

$\overline{x_1y_1z_1}.a_1=\overline{y_1oz_1}.a_n+\overline{x_1oy_1}.b_t+\overline{z_1ox_1}.c_t$
$\overline{x_1y_1z_1}.b_1=\overline{y_1oz_1}.c_t+\overline{x_1oy_1}.a_t+\overline{z_1ox_1}.b_n$
$\overline{x_1y_1z_1}.c_1=\overline{y_1oz_1}.b_t+\overline{x_1oy_1}.c_n+\overline{z_1ox_1}.a_t$

in which $a_1b_1c_1$ are the intensities of the

Fig. 14



components of the stress on the plane $x_1y_1z_1$ along xyz respectively. Now

$$\overline{y_1oz_1} \div \overline{x_1y_1z_1} = \cos xn$$
$$\overline{z_1ox_1} \div \overline{x_1y_1z_1} = \cos yn$$
$$\overline{x_1oy_1} \div \overline{x_1y_1z_1} = \cos zn.$$

$$\therefore a_1 = a_n \cos xn + b_t \cdot \cos zn + c_t \cos yn$$
$$b_1 = c_t \cos xn + a_t \cdot \cos zn + b_n \cos yn$$
$$c_1 = b_t \cos xn + c_n \cdot \cos zn + a_t \cos yn$$

and $r^2 = a_1^2 + b_1^2 + c_1^2$, therefore the resultant stress r is the diagonal of the right parallelopiped whose edges are $a_1b_1c_1$. In order to construct $a_1b_1c_1$ it is only necessary to lay off $a_n b_n c_n$, $a_t b_t c_t$ along the normal, and take the sums of such projections along xyz as are indicated in the above values of $a_1b_1c_1$.

Thus, in Fig. 14, let $x_1y_1z_1$ be the traces of a plane, and it is required to construct the stress upon a plane parallel to it through o.

The ground line between the planes of xoy and xoz is ox. The planes xoz and yoz on being revolved about ox and oy respectively, as in ordinary descriptive geometry, leave oz in two revolved positions at right angles to each other.

The three projections of the normal at o to the given plane are, as is well known, perpendicular to the traces of the given plane, and they are so represented. Let oa_z be the projection of the normal on xoy, and oa_y that on xoz. To find the true length of the normal, revolve it about one projection, say about oa_z, and if $a_z\, a_n = a_2\, a_y$ then is oa_n the revolved position of the normal.

Upon the normal let $oa_n = a_n$, $ob_n = b_n$, $oc_n = c_n$, the given normal components of the stresses upon the rectangular planes, and also let $oa_t = a_t$, $ob_t = b_t$, $oc_t = c_t$, the given tangential components upon the same planes.

Let $a_2b_2c_2$, $a_2'b_2'c_2'$ be the respective projections of the points $a_n\, b_n\, c_n$, $a_t\, b_t\, c_t$ of the normal upon the plane xoy by lines parallel to oz, similarly a_y, etc., are projections by parallels to oy, and a_z', etc., by parallels to ox.

We have taken the stresses c_n and c_t of different sign from the others, and so have called them negative and the others positive.

It is readily seen that the first of the above equations is constructed as follows:

$$a_1 = oa_1 = oa_2 + b_t\, b_z' - c_z'c_2'$$

Similarly, the other two equations become:

$$b_1 = ob_1 = -oc_2' + a_t\, a_2' + ob_2$$
$$c_1 = oc_1 = ab_2' - c_zc_t + oa_2'$$

We have thus found the coördinates of the extremity r of the stress or upon the given plane; hence its projections

upon the planes of refererence are respectively or_x, or_y, or_z.

PROBLEM 28.—In any state of stress defined by its three principal stresses, to find the stress on any given plane.

This problem is the special case of Problem 27, in which the tangential components are each zero. Taking the normal components given in Fig. 14 as principal stresses we find $oa_2 = a_n \cos xn$, $ob_2 = b_n \cos yn$, $oc_2 = c_n \cos zn$, as the coordinates which determine the stress or' upon the given plane, and the projections of or' are or_x', or_y', or_z', respectively.

From these results it is easy to show that the sum of the normal components of the stresses on any three planes is constant and equal to the sum of the principal stresses. This is a general property of solid stress in addition to those previously stated.

PROBLEM 29.—Any state of stress being defined by given simple stresses, to find the stresses on three planes at right angles to each other.

In Fig. 14 let a simple stress act along the normal to the plane $x_1y_1z_1$, and cause a stress on that plane whose intensity is $a_n = oa_n$, then is $a_n \cos xn = oa_2$ the intensity of the stress in the same direction acting on the plane yoz. The normal component of this latter intensity is

$$a_n \cos^2 xn = oa_2 . \cos xn = oa_3,$$

and it is obtained by making $oa_3' = oa_3$, $a_3'a_z'' \parallel x_1y_1$, and $a_z''a_3 \parallel oy$. The tangential component on yoz is od' in magnitude and direction, and it is obtained thus: make $a_z''d = a_z''a_3'$, then in the right angled triangle da_3a_z'', da_3 is the magnitude of the tangential component; now make $od' = da_3$. This tangential component can be resolved along the axes of y and z. The stress on the planes zox and xoy can be found in similar manner, since the tangential components which act on two planes at right angles to each other and in a direction perpendicular to their intersection are, as has been shown, equal; the complete construction will itself afford a test of its accuracy.

Other simple stresses may be treated in the same manner, and the resultant stress on either of the three planes, due to these simple stresses, is found by combining together the components which act on that plane due to each of the simple stresses.

It is useless to make the complete combination. It is sufficient to take the algebraic sum of the normal components acting on the plane, and then the algebraic sum of the tangential components along two directions in the plane which are at right angles, as along y and z in yoz.

The treatment of conjugate stresses in general appears to be too complicated to be practically useful, and we shall not at present construct the problems arising in its treatment.

A FEW NOTES ON METHODS OF BUILDING, AND MANUFACTURE OF MATERIALS, IN INDIA.

BY AN ASSISTANT ENGINEER, D.P.W., PUNJAB.

From "The Builder."

MATERIALS, their uses and manufacture, are often so different in India to those of Europe that it may possibly interest some of our readers to know the various kinds and values of timber; the method of manufacture of bricks and lime (generally very primitive), and other materials in use; and to know the many difficulties an engineer has to overcome, which arise purely from the scattered work he has to do, the scanty population (in many places) and means of transport, and other obstacles of an equally minute character.

This article does not aim at going very deeply into the subject, as those who wish to study the matter more closely cannot do better than by consulting "The Roorkee Treatise on Indian Civil Engineering," a book full of practical suggestions and descriptions of the uses and manufacture of materials, &c., in the

Bengal Presidency, besides the "Theory of Engineering," for which it is used as a text-book for the Roorkee College students.

BUILDINGS.

Stone.—The usual material is brick in the plains, and stone in the hills. It is only where stone is available on the spot that it can compete with brick, as the expense of carriage across unbridged torrent beds, and over unmetalled roads, is almost always a bar to its use in any but ornamental work. The red sandstones of the Salt Range, Delhi, and Jaipur, it is true, are carried a long way, but their use is confined to ornament alone, or to pavements of public buildings, and then only sparingly. The stone is all, or nearly all, sandstone, and generally good—in many places very good, and hard, but in others it is very poor, rotten, and worthless, except to be pounded up and mixed with lime. Granite is not found anywhere in the Punjab; neither is limestone used, except in the form of boulders for irrigation dams, &c., where massive work is required. In this form it has been extensively used at Madhopur, the head works of the Bair Doab Canal, where a dam across the Rair has been constructed, to drive the waters of that river, as they debouch from the hills, into the main canal. During the unprecedented floods of August and September, 1875, this enormous piece of work was undermined, and turned in many places completely topsy-turvey, giving ample evidence of the force of the waters, which at other places have spread ruin and desolation over the low grounds of the province.

Bricks.—The next material for the walls of houses of the better class is brick—"pucca" brick, as it is called, the word "pucca" meaning thorough, good, in contradistinction to "kucha," which means exactly the reverse, and is applied to sun-dried or unburnt bricks. The third or intermediate class is called "peela," and is applied to partially-burnt bricks on account of their color, "peela" being used for an ochre color, just such a one as an underburnt brick would have. All three kinds are used in different qualities of work, and in a dry climate, such as India, it is wonderful what a length of time an underburnt brick wall will last when properly protected with mud and straw plaster. Bricks are of all sizes; the old native brick was about 8 inches × 4 inches, and from 1 inch to 1¼ inches thick. These are in some places called "Akbare," possibly they were most common during the reign of Akbar (A.D. 1556–1605), under whom a large amount of work was commenced and partly completed. The native brick in common use now is called Lahore, and is about 5 inches × 3 inches × 1 inch. It makes very good strong work, but, as may be supposed, uses a good deal of mortar.

The bricks in use in the Department of Public Works and Railways are the English stock, 9 inches by 4½ inches × 2½ inches or 3 inches; the irrigation brick, which is 10 inches × 5 inches × 2½ inches; and the large brick, 12 inches × 6 inches × 2½ inches to three inches.

Kilns.—The old native kiln or "Pajarvah" is a very cheap though slow style of kiln, and the bricks have one advantage over flame kilns—they are thoroughly annealed. The kiln is V-shaped in plan, an excavation begun in the ground, and at a depth of 2 feet or 3 feet, is continued at an angle of about 1 in 10, until it merges into an embankment formed of the earth excavated. When these kilns were first started their dimensions were not very large possibly, but in many kilns whose lives vary from 20 to 150 years, the excavation at the toe of the V is from 3 feet to 10 feet above the surface of the ground.

The material used is brushwood, and horse or cattle litter, the solid refuse of the cities, &c. The method of loading is as follows:—A layer of light brushwood is laid at the bottom of the kiln, and covered with "oopla," or cow-dung cakes dried in the sun, leveled with litter, then a layer of bricks, two courses on edge of 9 inch bricks, or three of native bricks, and these are covered with litter double the thickness of brick below and damped down with ashes. This goes on until the loading has reached about 12 feet from toe of kiln, where, by the way, the firing begins. The courses of brick are here increased to three, and then four, and at a little distance two tiers of brick and litter are laid, and so on until the kiln is loaded well away

from toe; it is then fired. The kiln is always so placed as to face the prevailing wind, and when lit, the fire is driven forward by the wind. The kiln is set alight by igniting the brushwood at the mouth, and by damping down any place where the fire might burst out too freely, with ashes, the flame is kept in check. The loading proceeds, and as soon as the bricks near the mouth are cool, unloading the kiln is commenced, and in this way unloading is going on at the mouth, firing in the center, and loading towards the end. A large "Pajarvah," 50 feet long, will contain an equivalent to 300,000 of 9 inch bricks, and will take seven or eight months loading and unloading.

The "*Clamps*" present additional facilities for unloading. They vary in size, from the one which contains 20,000 bricks (9 inch size), to the large one which contains 150,000. The fuel is "oopla" (dry cow-dung cakes, about 8 inches diameter and conical, 3 inches or 4 inches or 5 inches in height). The loading is horizontal, with a perceptible dip towards the center. The proportions of fuel are: 1st layer, 2 of fuel to $\frac{3}{4}$ brick; 2d or 3d layer, 2 to 1; and above that less and less, until near the top it is $1\frac{1}{2}$ to 1. A small kiln will turn out bricks in three weeks from firing, and a large one in six weeks. Bricks burnt in litter kilns do not burn so deep a red as those from flame-kilns; they are much harder and better annealed, but they contain more ammonia and discolor much sooner. Clamps are very useful in burning ornamental brick, as the heating and cooling process is so gradual that the fine edges or mouldings are very little injured.

Flame-kilns.—There are a good many varieties of flame-kilns; one or two successful patents have been, within the last four or five years, obtained for their use. The best kiln of the old kind is called the "Lind" kiln, and is about 26 feet by 18 feet, inside measurement, and 12 feet high above the arches. An excavation in the ground, 7 feet in depth, contains the furnace, the same size as the kiln, but divided into two by a wall in the furnace. There are parallel walls 5 inches or 6 inches apart, carried on arches, on which the bricks to be burnt rest. In this kind of kiln large and small wood can be burnt together, even large logs of 1 cwt. to 2 cwt. When properly loaded and fired, the loading occupies two days' firing, 70 to 80 hours, according to season of year. The bricks cool in 25 days, and give an out-turn of 88 to 94 per cent. Each kiln contains 44,000 to 47,000 9-inch bricks.

Other varieties of the same-sized kiln are used; in many there are no arches, the bricks themselves forming arches. That called the "Allahabad" kiln is about 100 ft. × 18 × 12. Its method is rather complicated, and wood, coal, and charcoal are all used during the process. It burns a large quantity of bricks at a time, between 2 and $2\frac{1}{2}$ lakhs, and its out-turn is said to be very good. Coal is not much used in Upper India, and nowhere above Allahabad, owing to the great cost of carriage.

The kiln which is best adapted for large works is the one known as "Butt's Annular Kiln;" it is a very similar one in theory to Hoffman's, but is much simpler, and not nearly so costly to erect. It requires considerable experience before its full capabilities can be developed. The coolies in charge must be all trained men—otherwise it is a failure. The principle of the kiln is simply one which may be called "endless." There are two walls, circular on plan, 12 ft. apart, and having 11 flights of steps, which serve as buttresses, whilst giving access to the top. The bricks are loosely packed in concentric walls, 3 in. or 4 in. apart, and at every four feet arches are constructed, exactly opposite the fire-holes in the external walls. The radius of the inner wall is 75 ft., and each section—*i. e.*, the piece between a flight of steps—contains 8 holes. The method of loading is peculiar, and not easily understood without a diagram. Suffice it to say, that four holes are fired with wood (not over 8 in. diameter) at the same time, and the smoke is drawn out of openings left in the loading, about 20 ft. ahead of the last hole, air being drawn through the already fired part of the kiln. By this means the green bricks are gradually dried, heated, and brought to a white heat, and as gradually cooled after they have been burnt, as there is no escape of heat upward, the top layer being covered with one ft. of ashes. A lakh (100,000) of bricks can thus be burnt with 150 ohms or 5.36 tons of fuel.

A kiln is divided into 12 divisions ; each division, being about 50 ft. in length, contains 23,000 9-in. bricks. It follows that by the time the kiln has been once fired round 276,000 bricks will have been burnt ; about 14 to 15 sections can be fired per mensem. Only about two-thirds of the kiln is loaded at one time—say, about 200,000 of bricks ; unloading goes on at one section, loading a section or two behind, and firing from half to a section behind that, so that even though the loading be interfered with by unseasonable weather, the out-turn can be depended upon for some weeks. The kiln described is circular on plan, but it could, of course, be built elliptically equally well to suit shape on size of ground. There is also a rectangular kiln on the same principle, but it is not one much used. Size of ground is not usually an object of importance as brickfields are, as a rule, some distance away from cantonments or stations, on sanitary grounds. Underburnt bricks are much used in native buildings and in partition walls of 2d class buildings, as they stand very well when not exposed to the atmosphere or damp. Sun-dried bricks are used in very large quantities, both in native buildings and in those built by Government for jails, &c., in dry climates. When properly plastered (mud, chopped straw, and cowdung) and kept in repair, the heavy rains have very little effect on them, but now and then a shower of rain of long continuance will bring the houses down as if they were made of sugar. It is said 8 hours' rain would not leave such a place as Mooltan. During the season of 1875 the whole of the new jail in Amritsur and part of that in Lahore were completely ruined. Both were built of sun-dried brick, and both together represent a loss of some £12,000 to Government. In the author's opinion sun-dried work for Government buildings is quite a mistake, and, though cheap, is very nasty. It cannot be repaired as often as it should be, and, in the long run, costs a great deal.

Concrete is the material for India. What this country wants is a good quick-setting cement like the Portland, and that it has not as yet got. Bricks being obtained, the next requisite is

Lime.—Stone lime is obtainable near the hills, and the average distance from the places where it is made and its destination is, in the Punjab, 30 to 50 miles. It is in many places obtainable only in a slaked condition. Its cost unslaked varies from £1 to £4 per ton. It is generally of a white color, and fat. In fact, the inferior and more hydraulic qualities are not much used, as they could bear less admixture of soorkee, the cheaper material. The limestone is found in the bed of hill torrents, and is washed down from the mountains above. It is never grained, and the boulders are always burnt rough just as they are found. The kilns are V-shaped, and are loaded with the fuel underneath, and are then left to burn themselves out. The fuel is the light brush-wood of the hills burnt quite green. The result is that only about half is burnt properly, and each large lump has a core of imperfectly calcined stone in its interior, which is pure waste, as the lime is always purchased by weight. Fat lime is generally used with soorkee, which is brick refuse pounded fine, screened, and then mixed with the lime in the proportion of 1 lime to 2 soorkee. The latter is a puzzolana, and should be made from thoroughly burnt bricks. Sand is not often used as it cannot be obtained coarse enough, and is, besides, full of mica. It is sometimes mixed with a proportion of soorkee to prevent cracking in plaster, &c.

Kunkur is another lime-producing substance. Kunkur is, it is believed, found only in India, and is generally supposed to be produced by the filtering action of water through coarse soil. The water, of course, contains particles of lime. These are deposited sometimes on the surface and sometimes below the surface of the ground. It is always found in larger quantities near the hills, and at the sides of old water channels than anywhere else, and at Pathankote. About 5 miles from that town there are several places where kunkur is found on the surface, with evident marks of its having been formed around vegetable substances —for instance, a kind of stalagmite, formed around a stalk of grass or reed —and one specimen was shown to the author which distinctly showed that it had been at one time the outer case of a gnarled base of a tree, the impression of the bark being distinctly traceable. The usual kind found is about the size of po-

tatoes, in lumps, in which earth is more or less mixed, but it is also found having the appearance of stone, in layers from 2 to 4 feet thick, and about 3 to 5 feet from the surface of the ground. About Aligurh it is used as a building material, and it has one peculiarity that it hardens rapidly on exposure to the air. From an analysis of kunkurs, near Goordaspur, it appears that the average percentage of carbonate of lime in the specimens was 50 or 51 per cent., and those about Sealkote, 52 to 53. This shows that kunkur is a natural cement, and, though it is not a quick-setting one, it is, nevertheless, a fact that it is considered and treated as a lime by most engineers. Some of the kunkurs require an addition of fat lime, and some of soorkee; but unless the lime is burnt under strict supervision it is very frequently adulterated, and it is now almost universally burnt with charcoal instead of with cow-dung, as it used to be. The usual way has been to load it into clamps with oopla for fuel, but when charcoal was used V-shaped (in section) kilns were introduced, in which the kunkur was either mixed with the charcoal in proper proportions or else in alternate layers of kunkur and charcoal, the lighting being done by igniting pieces of charcoal and then pushing them into vents left at the bottom of the kiln, previously fitted with either charcoal or oopla. The out-turn was fairly good, but kilns of large size could not be employed owing to the precarious nature of the out-turn; sometimes a high wind or fall of rain would either burn a kiln to clinker or make it under-burnt. A plan has been recently adopted which was entirely successful—viz., the clamp system, but with charcoal fuel. Very good results were obtained, and the kiln could be fired when required, or if fired could be protected with mud plaster, until it was necessary to open it. When burnt the nodules are pounded fine, and should be used with a very small amount of water, and mixed with that only just previous to use. The common practice, however, is to mix it with a good deal of water, and to leave it, sometimes for a day or two. In the writer's opinion, this simply ruins it, as he considers it a cement, and not a lime. Pure cement simply laid in a mould and not rammed will, in most cases, harden under water if left to harden in the air for 48 to 72 hours previously. In concrete it makes excellent work, and it has a very nice appearance owing to its reddish grey color.

Floors.—There are, in Indian houses, no second floors—at least, in the upper provinces—and very few barracks have them, so that the floors are, of course, placed directly over the earthen filling in of plinth. They are, as a rule, in Government buildings, of bricks or square tiles 3 inches thick, laid over either a concrete bed 4½ inches thick or over a course of bricks and bats. In private houses they are seldom anything but coarse mortar, hardly to be called concrete. In double-storied barracks the upper floors are 1½ inch planks nailed to joists carried on beams or trusses.

Roof Coverings.—The roofs may be said to be divided into two divisions—flat and sloping. Flat roofs are by far the most common, and trussed roofs are only adopted in large public buildings and barracks for European troops. Flat-roof coverings are usually of the following materials in the North-west provinces:—1st, a course of 12 inches × 12 inches, or 12 inches × 6 inch flat tiles, 1½ inches to 2 inches thick, and over this 4 inches of well-beaten "terrace," which is concrete or coarse mortar floated on the upper surface with pure white lime mixed with "goor," or coarse sugar. This is very liable to crack owing to the tremendous power of the sun in the hot weather, and the cooling action of a sudden storm of rain. These hair cracks are a constant source of annoyance and leakage, and require to be constantly filled up with rosin and lime or Portland cement. In the Punjab the flat-roof coverings are of 12 inches × 6 inches × 1½ inch to 2 inch flat tiles covered with ½ inch to 1½ inches plaster, well beaten, and 4 inches of earth well beaten, covered with 2 inches of mud plaster, or a composition of mud, chopped straw called bhoola, and cow-dung. These roofs are very cool, but require to have the weeds pulled up before the annual rains, and then replastered. If this is properly done the roofs never leak. A cheaper kind is made by laying thin boards over the joists and then loose bricks and mud as above. Stables and out-houses, also the ordinary bazaar-house roofs, are of

reed mats called "sirki," covered with a coarser reed called "sirkunda," with the mud above that. When beaten, plastered, and kept in proper repair, they do not leak much, but the white ants are, of course, very troublesome, and the reeds have to be renewed every 7 or 8 years.

Tiled roofs are made of flat and half round tiles, and over the joists there are 12 inches × 6 inches flat tiles, covered with 1 inch plaster. Over this the tiles called "Goodwyn" tiles are laid. These are those just spoken of, and 200 are required to cover an area of 100 square feet. The flat tiles are about 14 inches × 12 inches and 1 inch thick, having the sides turned up 1 inch. They are placed side by side in a little fresh mortar, and the half round tiles are then laid in mortar over the abutting joints.

The "Jubbulpur" or "Allahabad" tiles are similar in idea. The former are merely smaller tiles, one set being laid over the other, forming a double roof, very cool it is said, and the latter are the same with the exception of the lower half round, which are demi-hexagons, to enable 2 inches course of flats to be laid evenly and to avoid slipping. Italian tiles are very little used, as also slates: —1st, on account of their cost; 2d, on account of the heat, they being no protection whatever; and, 3d, on account of their being no protection from tropical rain. Slates and shingles are used in the bricks in double layers, where they serve their purpose very well.

Thatching is not now much resorted to, owing to the mutineers in 1857 having set them alight as a first measure towards creating a disturbance. They make the coolest of any roof coverings. Slabs of stone are used in the central provinces at Saugor, but hardly anywhere else.

FOOD *vs.* FUEL—CALCULATION OF THE NECESSARY FOOD FOR A HORSE AT WORK.

By M. BIXIO, President of the Compagnie General des Voitures, Paris.

Translated from "Revue Industrielle" for Van Nostrand's Magazine.

It is evident that the quantity of food required by a horse depends upon two conditions: his weight, and the work he performs. Upon his weight first, because in order to keep him in good condition, it will be necessary to supply the losses arising from respiration, perspiration, and his internal functions; upon the work that he performs, because all work produces heat and this occasions loss of weight.

In considering the conditions of the life of the animal, we may count three different states: 1st. That in which he does nothing: 2d. That in which he moves about but performs no work: and 3d. That in which he does some kind of work.

The food necessary for his maintenance under these conditions separately we will designate in order: The Ration of sustenance: The Ration of Transportation: The Ration of Work.

The Ration of Sustenance is the food necessary to keep him in good condition, supposing that he remains in the stable.

The Ration of Transportation is the amount of food in excess of the preceding ration necessary to keep up his condition if he moves about without hauling or carrying any load.

The Ration of Work is the amount of food in excess of the two preceding amounts, required to enable the animal to perform some useful work.

We will proceed to show how we can arrive at a determination of the amounts of these several rations, and then will establish a general formula.

A food unit is a necessary basis of such calculations, and the science of physiology must supply our want. It is necessary to determine among the mixture of nutritive elements of the food what ones, by their combination with oxygen in the blood, disengage the heat which is the source of the vital force necessary for the muscular contractions.

From investigations upon this subject made in Germany, England, and France, the conclusion has been reached that the nitrogenous or protein compounds are chiefly instrumental in producing the effect in question. The kilogram of protein has, therefore, been taken as the alimentary unit.

M. Sanson, Professor of Zootechnic, at Grignon, adopting this unit has arrived at the following equation:

$$P=\frac{T}{C}$$

In which P is the protein necessary in a ration, T is the work performed, and C is the kilogrammeters of work produced by a kilogram of protein.

The well known formula of mechanical work is

$$T=F.E$$

in which F represents the force exerted and E the path described. We know also that the force exerted in hauling a load is equal to the load moved, multiplied by the coefficient of traction.

If now we designate by M. the weight of the horse, and by A the quantity of protein necessary to sustain 100 kilograms of his weight when at rest; then if p be the ratio of sustenance we shall have

$$p=M\times 0.01A$$

To determine the work produced by the horse in transporting his own weight to any given distance, we employ the formula T=FE. In this case F is the weight of the animal M, increased by m the weight of his harness, and multiplied by .01B. B being the coefficient of transportation, or the effort necessary to keep in motion 100 kilograms of weight. We have then

$$F=(M+m).01B$$

or

$$T=(M+m).01BE.$$

If we represent by p' the protein of the ration of transportation we shall have in the formula of M. Sanson $P=\frac{T}{C}$

$$p'=\frac{(M+m).01BE}{C}$$

in which m is the weight of the harness or of saddle and rider if the horse carry such.

If now we represent by p'' the protein consumed in performing useful work; by N the weight of the carriage; D the coefficient of traction or the effort necessary to draw 100 kilograms of weight along the proposed road; the formula for work becomes

$$T=N\ .01\ DE$$

and Sanson's formula becomes

$$p''=\frac{N\ .01\ DE}{C}$$

Uniting in a single formula the three different formulas above we have

$$P=p+p'+p''$$

whence by substituting the values determined we get

$$P=M\ .01\ A+\frac{(M+m).1\ BE}{C}+\frac{N\ .01\ DE}{C}$$

in which the three different rations are represented in succession.

This reduces to the form

$$P=.01\left(MA+\frac{E[(M+m)B+ND]}{C}\right)$$

Such is the general formula for determining the quantity of protein for a horse when at work.

If the animal works only on alternate days, then he requires his sustenance ration and so much of the ration of transportation as will supply his necessary movements about the stable or pasture. If the sum of such movements be represented by E′ then the ration for a day of rest would be

$$P=.01\ A+\frac{M\ .01\ BE'}{C}$$

or

$$P=.01\left(MA+\frac{MBE'}{C}\right)$$

The general formula then for the protein required for two days, one of work and one of rest, is

$$P=.01\left(2MA+\frac{E[(M+m)B+ND]+MBE'}{C}\right)$$

which may be written

$$P=.01\left(2MA+\frac{M(E+E')B+E(mB+ND)}{C}\right)$$

In this formula

P=the protein necessary for two days.

M=the weight of the horse.

m=the weight of his harness.

N=the weight of the carriage.

A=the coefficient of sustenance.

B=the coefficient of transportation.

D=the coefficient of traction.

C=the mechanical equivalent of a kilogram of protein.

In order that this formula shall be of use the values of the coefficients A, B, C and D must be determined. This is an object of importance in our industry.

It is necessary to remark here that the above formula is based on the idea that nitrogenous materials in the food are necessary for the production of force.

This theory is disputed by M. Voit, who claims that the consumption of nitrogen is no greater in working than resting, while the combustion of carbon and of hydrogen is greatly augmented.

Prof. Hervé Mangon proposes to establish a formula based on the following facts:

"An animal is a machine for combustion. His food is the fuel and his voidings are the ashes. Analysis of the fuel, and the ashes determines what and how much has been burned."

"The burnt portion contains a determinate amount of carbon and hydrogen, which in burning have produced a definite number of heat units."

"The number of heat units multiplied by the mechanical equivalent of heat will give the theoretical number of units of work in kilogrammeters."

"This, multiplied by the proper coefficient, gives the result in units of work obtained.

In working upon this basis Prof. Magnon remarks that the difference between the winter and summer rations may be taken in account.

This idea of establishing a formula is based on the mechanical theory of heat, and the above propositions indicate that observations and experiments upon the animals themselves are of the first importance.

This is not merely a solution of a purely scientific problem but one of great practical utility to an important industry. It is to determine how we shall best nourish our horses so that they perform their work at the least expense.

The nitrogenous elements of food are the most costly ones and we shall economise if we can obtain the requisite force from the carbon and hydrogen only.

But it may be urged on physiological grounds that nitrogen plays an important part in sustaining the animal, and our general formula, taking account of sustenance, calls for a certain amount of protein; only it may be modified perhaps by determining how much carbon and hydrogen are necessary to produce the useful effect T.

The values of the coefficients in our formula remain yet to be determined.

It is generally admitted that for the purposes of sustenance 30 grams of protein are required for each 100 kilograms of weight of body. Therefore A in the formula represents $0.03k$.

In some experiments upon carrying loads M. Sanson concludes that for the horse a constant effort of 10 kilograms is necessary for each 100 kilograms of weight carried at a trot. B in the formula would therefore equal 10.

Morin's experiments upon traction on roads give—for a coefficient upon a dry pavement, 6 per 100 drawn at a trot and 3 per 100 at a walk. Upon the hypothesis of working at a trot the coefficient D would be 6. From experiments by M. Plessis, an engineer in our employ, made upon our own vehicles and upon the several routes, it would seem that this coefficient 6 is too high by nearly one half.

Finally the coefficient C the most important of all has been a matter of research by M. Sanson, who concludes that one kilogram of protein ought to produce 1600000 kilogrammeters of work.

Consequently C=1600000

We believe for our part that this coefficient which has been calculated from the work of omnibus horses is too high.

We find in Prof. Mangon's work: (Traité du Genie Rural) a calculation which assigns to 258 grams of oats a useful effect of 100000 kilogrammeters. It was obtained by observation of agricultural horses working at a walk.

To produce 1600000 kilogrammeters of work would require $4^k.128$ of oats containing 462 grams of protein; less

than half the amount determined by M. Sanson; but it must be remembered that this latter figure is based on working at a walk.

On the other hand we find in the same work, that for the *Cheveux de poste* of Paris that 1 kilogram of oats is required for 100000 kilogrammeters of work. This is equal to $1^k.798$ of protein for 1600000 kilogrammeters, which is much more than M. Sanson's estimate. We see from these different estimates how important it is that we should determine by careful experiment the conditions of our particular service.

Suppose we have to determine the ration of a horse drawing our coupé No. 4 for one day and resting the next. The mean weight of the vehicle and load (carrying from one to three passengers) is 533 kilograms. The mean weight of the horse is 420*k*; the harness weighs 14*k*. The route is about 50 kilometers. The horse during his day of rest does not move more than 300 meters.

The equation for rations, making the substitutions, becomes

$$P.01=\left(2420\times.03+\frac{50300\times420\times10+(14\times10+533\times6)\,50000}{1600000}\right)$$

which reduces to

$$P=2^k,\ 364.$$

This is the quantity of protein necessary to give a horse in two days when he works one of them under the above conditions.

If oats alone, (containing 7.93 per cent. of protein) are given to the horse the gross weight of the ration would be $28^k.423$. But other food such as hay, corn, bran, etc. etc., is necessary.

We will suppose there is given to the horse during the two days

5 kilos. hay containing .5055 of protein.
5 kilos. straw containing .1818 of protein.
0.4 kilos. bran containing .0553 of protein.

Total $0^k.7426$ "

This would render necessary for the protein of the oats only

$$2^k.364-0^k.7426-1^k.6214$$

which corresponds to a weight of $20^k.446$

In our tables of rations actually given to our horses (Nov. 1877) we estimate the protein at $1^k.6892$ which would correspond to a weight of oats $=21^k.301$.

We feel assured that our equation has a practical value but that for *general* use, it will be necessary to establish the values of the different coefficients separately for the different kinds of work which horses are required to perform.

Some further experiments are necessary to obtain precise values of the coefficients for the varying conditions of our own service.

But in the above analysis, we have determined the question—upon what basis a good ration should be established, and what elements are to be considered in the calculation.

In treating fully the second part of this question, it will be necessary to determine not only the protein but the proportionate quantities of the other constituents of the food. This would require two more equations to determine exactly the conditions of a good ration.

BUILDINGS AND EARTHQUAKES.

From "The Building News."

ALTHOUGH in this country earthquakes are happily rare, we know that in southern and eastern lands they are of such frequent occurrence that the architect has to take the stability of his structures into serious consideration. Indian and Eastern architecture generally has been considerably modified by conditions due to this cause, and we know that the Italian mediævalist introduced so largely the tie into his arched openings as to sacrifice, in great measure, the motive and beauty of the pointed style. Japan has especially suffered from visitations of earthquakes, and it is not surprising that the engineers and others engaged in con-

struction should pay special attention to the means best adapted to overcome the shocks to which buildings are exposed.

We have before us two pamphlets by Mr. John Perry and Mr. W. E. Ayrton, Professors of Engineering in the Imperial College of Tokio, Japan. In one of these* the authors investigate the effects produced by an earthquake on a structure, especially with regard to the time of vibration. Generally it has been assumed that the shock caused by an earthquake produces an impact upon a building, but recent inquiries have shown that it is a wave of elastic compression in any direction, vertically or horizontally, through the earth's crust. These waves of undulation, if we may so call them, are no doubt transmitted to the surface in a modified manner owing to surface irregularities, such as mountain ranges and geological structure. Rocky strata, of course, transmit them rapidly. But we have to regard an earthquake as an elastic compression in some direction. This being so, it follows that a building is affected by an undulation, or rather participates in the vibration of a point of the earth's surface, which vibration may be mathematically determined, or at least approximately so. If we imagine such a wave of vibration to pass under a large building, such as the Law Courts for example, it is obvious some portions of the structure would be affected in a greater degree than others. The lofty square towers would vibrate slowly, compared with the lower parts, and according to the relative height and homogeneity of the masses would be the amount of vibration each part would share. For instance, in a low building we may fairly assume the time of vibration of the shock and of the structure to be approximately equal, if the parts are of the same density; but if the building is lofty it will vibrate more slowly. A slowly vibrating structure is necessarily subjected to stresses of a complicated kind, and more severe than those of a quickly vibrating one. It is not difficult to comprehend the truth of this proposition, and Messrs. John Perry and W. E. Ayrton have shown that the stability of structures subjected to earthquakes depends mainly upon the quickness of their vibration, or, in other words, on their rigidity of structure and lowness. A slowly vibrating structure—that is to say, a lofty building—will probably, as our authors say, "get broken in its connections with the foundations, if these be rigidly fixed to the ground; consequently (and we must here oppose the practice of many architects and engineers) putting a heavy top to a lighthouse, the chimney of a factory or other high building, must certainly take from its stability." As they observe, "it is the relative velocity of the base of the structure, with regard to the other parts, which is the fixed quantity, and therefore that the more massive the structure, the more momentum enters it through the base." An ordinary Japanese two-storied house, with its heavy roof, it is supposed, takes four seconds to make a complete vibration, the restoring forces which bring the structure back to its normal position being due to stiffness of the joints, and to the fact that the house is not rigidly connected with the ground. It will surprise the English architect to learn that the Japanese houses are without the foundations we are accustomed to use; the vertical posts rest on detached stones, and there are no diagonal braces. Thus the building can be displaced from its position of equilibrium by any shock without fracture occurring. There is a "viscous resistance," as the authors term it, to the motion, caused by the various joints, and such resistance diminishes the motion and adds to the safety of the building. Particular stress is laid on this viscous resistance of the joints, and also to the absence of diagonal pieces to lessen the strains. The Japanese temples are considered pretty secure against shock, as they are buildings of slow vibration, and have a great deal of viscosity in their joints. It must be borne in mind that a rigidly connected foundation is independent of the mass of building, and the shock tends to displace at any weak point or surface of contact between different portions. All non-homogeneous buildings have some parts only capable of slow vibration compared to others. The authors justly say that there is a best method of constructing buildings in an earthquake country: this obviously consists in constructing the lower parts of the

* On Structures in an Earthquake Country. By John Perry and W. E. Ayrton, Professors in the Imperial College of Engineering, Tokio, Japan.

building with yielding material, so that the shock from an earthquake may be reduced in intensity and the vibration of the upper part diminished. A rocky or rigid foundation, on the other hand, transmits the vibration or momentum undiminished to the upper parts. Again, a foundation of yielding timber or some soft elastic substance would form a cushion by means of which the time of transmission of the momentum due to the shock may be increased. The authors point out it is desirable to keep houses built of ordinary wall thicknesses, with brick and common mortar, as low as possible—at most not more than two stories high; but if good cement be employed instead of bad mortar then their height may be safely two or three stories. Another point is the horizontal vibration of the ground. This causes a kind of shearing stress in the joints which mortar cannot transmit, and it is desirable, therefore, to make the joints rigid in cement so that the walls may resist a sliding as well as a crushing stress. No doubt we have here a strong argument in favor of cement concretes for building walls in earthquake countries. At any rate it is laid down that the most suitable structures for these contingencies, if of stone, are those built of large stones set in good cement with walls of considerable thickness at the base, diminished gradually in proportion to the mass and height of the building, and we have a strong presumptive argument in favor of pyramidal buildings. As timber has greater tensile resistance to shock, and as the mass of timber in a building is small, a building of this material is even more desirable if constructed with strong joints, while wrought iron and steel have still stronger claims in these respects. Another hint is given—namely, that timber structures should not be too rigidly fastened to the earth. Without going into the calculations of the times of vibration of different buildings given by the authors, as regards shape and height, it is obvious the conclusions drawn by them are convincing; and that, to insure stability in structures liable to shocks, the relative vibrations of the parts of the structure of any given material must be taken into account. Thus, high chimneys, such as many engineers have erected recently, crowned with heavy cornices, are unsafe in a country like Japan: for, as the authors show, the period of natural vibration of a chimney 150 ft. high and 10 ft. square is about 2¼ seconds—a period much too slow to be safe when connected with the walls of a building of less height and consequently of less vibration.

We here turn to another very interesting paper read by the authors before the Asiatic Society of Japan,* in which the motion caused by an earthquake is investigated. The principle our authors set out with is that it is possible to read an earthquake message by the motion of a body attached to the earth by springs. Thus "the centre mass of a body fastened by means of springs inside a metal box rigidly attached to the earth has in certain cases motions with respect to the box itself which in miniature with great exactitude represent the motions of a point of the box during the earthquake." Here we have a self-evident principle upon which an apparatus for recording vibration can be constructed. Without diagrams it is difficult to convey a correct idea of the seismometer of Messrs. Perry and Ayrton. But we may describe it briefly as a strong iron case rigidly fixed to the rocky crust of the earth, with a leaden ball of 400 lbs., supported by five strong spiral springs, four of which are horizontal and one vertical, all having the same period, so that if there were no friction the ball would describe an ellipse when freely vibrating. To record the different horizontal movements there are three arms with pencils; these are made to press by means of spiral springs on a band of paper moved horizontally by clockwork. By these and other means an automatic register of the motion of the earth is diagrammatically made, and these diagrams assume irregular spirals on the paper. Thus the position, velocity, direction, and acceleration of the ball at any moment is recorded, and therefore the motion of any point upon the earth's surface is also registered. Professor Palmieri and others have invented electro-magnetic seismographs, to record earthquake vibrations and intensities, but the exactitude of the records made has been questioned by Mr. Mallet and other authorities in the science of

*On a Neglected Principle that may be Employed in Earthquake Measurements.

seismometry. We may simply add that the authors propose to place three of their instruments on the plain of Yedo, with clocks in telegraphic communication, by which means the vibration and motion of an earthquake-wave could be determined. We only hope the ingenious authors will be assisted in their experiments by the Japanese Government, and that facilities to perfect their instruments will be afforded them in the interests of science and humanity.

THE ACTION OF BRAKES.

From "English Mechanic."

THE remarkable and unexpected results obtained during the elaborate experiments with railway brakes, made a few weeks ago on the London and Brighton line, formed the subject of the paper read by Captain Douglas Galton, at the meeting of the Institution of Mechanical Engineers held in Paris. These experiments form the first of a series which it is intended to make with the view of ascertaining (1) the actual pressure required to produce a maximum retardation of the revolving wheels at different velocities; (2), the actual pressure exerted by the different forms of continuous breaks now in use; (3) the time required to bring the break-blocks into operation in the several parts of the train; and (4), the retarding power of the existing continuous brakes, tested on trains running under similar conditions of weight and speed. From the enumeration of these heads it will be readily understood that when completed, we shall have the most important contribution to the literature of the brake question which has hitherto been made; and the first instalment, contained in Captain Galton's paper, is sufficient evidence of the probable value of the series. The experiments described were undertaken to ascertain the co-efficient of friction between brake-blocks and wheels and between the wheels and rails, both when the wheels are revolving and when skidded. It is scarcely necessary to insist on the importance of ascertaining by actual test the exact value of a co-efficient upon which the whole system of brakes depend; and the engineering world is much indebted to the London and Brighton Railway Company for the manner in which they have taken up the question and facilitated the carrying out of the experiments. The experimental van and the recording apparatus were designed and constructed by Mr. Westinghouse and Mr. Stroudly respectively; but for our present purpose it is unnecessary to give a description of the means taken to obtain the results. The latter are unquestionably as correct as ingenuity and care could make them, and if they are remarkable, they serve to show that it is the unexpected that always happens. The experiments under notice were made at the end of May near Brighton, the first day being dry the second stormy, and the third fine, with showers. There was thus a sufficient variety of weather to render the experiments of more value than they might have been if made under uniform conditions, but there was not time to collate all the results before sending in the paper. Captain Galton, therefore, exhibited only a few of the diagrams taken, but these were of so remarkable a character as to excite the keenest attention of the engineers present. In experiment No. 15, May 28th, the brake-van was slipped when traveling at the rate of 40 miles an hour. The pressure on the brake-blocks remained nearly constant during the experiment, and being greater than that required by the co-efficient of friction between the brake-blocks and wheels due to velocity, the friction increased so rapidly as to cause the wheels to skid immediately. The friction at once decreased rapidly, but rose again as the speed diminished, attaining the maximum as the train came to rest, which it did after many jerks in 12½ seconds. In experiment No. 16, May 28th, the van was again slipped—the speed being 46 miles. The pressure of the air was less than in the previous

experiment, and it was gradually diminished during the experiment; consequently the pressure on the blocks was correspondingly reduced. At first the friction between blocks and wheels decreased slightly, but, when the velocity diminished the friction increased rapidly and the van came to rest without a jerk in 12 seconds. Thus the quicker stop was made by the revolving wheels which originally were traveling at a higher speed than in the case of the skidded wheels. This effect was exhibited in a decided form by experiment No. 3, May 28th, in which the speed was 44½ miles. The pressure applied to the blocks was sufficient to skid the wheels at once, and the diagram shows that the co-efficient of friction between the blocks and the wheels decreased immediately after the skidding and did not rise until the end of the experiment, while tractive force on the draw bar, at first increased by the act of skidding, largely decreased as soon as the wheels were held by the blocks. In experiment No. 3, May 29th, the engine and van were brought to rest from a speed of 39 miles an hour. The air was allowed to escape from the cylinder through a small hole after the the brakes were applied, so that the pressure decreased during the whole experiment. The diagram in this case shows that the retarding force due to the pressure of the blocks was at first diminished until the reduction of velocity reached the point where the increase in the co-efficient of friction was sufficient to overcome the effect of the diminished pressure applied to the blocks. At this point the retarding effect was increased, and the wheels were skidded. The curve immediately rose in a nearly vertical line showing that the co-efficient of friction became very great as the wheels came to rest—the time during which the wheel was partly rotating, partly slipping being almost inappreciable. Immediately after the rise, the curve fell to a point far below its original position. Thus showing that with skidded wheels there is a great diminution in the retarding effect of the brakes. As the velocity continued to decrease the curve steadily rose, thus showing that the co-efficient of friction between the rails and skidded wheels increases as the velocity diminishes. At the moment of coming to rest the co-efficient of friction became very great. The results obtained in these experiments may be taken as a fair sample of the series; from which we learn that the application of brakes to wheels does not appear to retard the rapidity of their rotation, but when it falls below that due to the speed at which the train is moving, immediate skidding is almost inevitable. The resistance resulting from the application of brakes without skidding is greater than that caused by skidded wheels. During the moment of skidding, the retarding force increases enormously, but immediately afterwards falls to less than that what it was before skidding. The pressure required to skid is much higher than necessary to hold the wheels, and appears to have a relation to the weight on the wheels themselves as well as to their adhesion and velocity. On this point Captain Galton says:—"It would seem that the great increase in the frictional resistance of the blocks on the wheels, just before and at the moment of skidding, due to the increase in the co efficient of friction when the relative motion of the blocks and the wheels become small, is what destroys the rotating momentum of the wheel so quickly". With constant pressures the friction between the blocks and the wheels increases as the velocity decreases, until, as the experiments proved, the wheels are skidded. But it was also discovered that in order to obtain the maximum retarding effect the wheels ought never to be skidded, but the pressure on the wheels should at all times be just less than is required for skidding. In order to effect the desired result, then, the pressure between the blocks and wheels ought to be very great when first applied, gradually diminishing as the train comes to rest. Such an outcome from these experiments discloses the fact that all the hand-brakes, and most of the continuous brakes, have been designed to suit conditions which do not exist in practice. The old saying —you can do no more than skid—is shown to be utterly erroneous, and the most successful brake is that one, the inventor of which has unconsciously as it seems, grasped the true principle.

That the skidding of wheels is not the best way to stop a train has been known and urged persistently by some railway

men, and the drivers and guards on most lines have orders to release the brakes when the wheels skid; but, until these experiments demonstrated the fact, not a few drivers and others, engineers amongst them, firmly believed that the skidding of the wheels was the readiest method of stopping. It has been objected to mostly because of the wear of the tires—flat places being highly objectionable. So long ago as 1846 Mr. Gooch, while connected with the South Western Railway, issued a rule to his men that wheels were not to be skidded, and if skidding did take place the brakes were to be immediately released and applied again. Mr. Tomlinson said that every practical engine-man knew that the skidding of wheels was a great mistake; but we venture to think that Mr. Tomlinson need not travel far to find plenty of practical engine-men who would argue the point with him. The gentleman who preceded him in the discussion, Mr. Haswell, expressed his surprise at the results of the experiments described by Capt. Galton, as the Newark trials had led the commissioners to form a contrary opinion as to the value of skidding. Mr. Brown, of Winterthur, speaking from practical experience on lines of heavy gradients in Switzerland, declared that if the wheels were skidded much of the retarding force was lost. Mr. Yeomans said that when the vacuum brake was first applied on the Metropolitan a vacuum of 15 inches (?) was found to skid the wheels. The drivers were, therefore, ordered not to exceed twelve inches. He controverted the opinion that the greatest pressure ought to be applied first, and thought that a sudden application of brake-power destroyed the wheels. Unfortunately no reasons were offered for these opinions, save that Mr. Yeomans had seen wheels that had been destroyed by the sudden application of the Westinghouse brakes. He considered that Capt. Galton's experiments had only confirmed what was well known, and that, to obtain any useful information, experiments extending over many years of actual service were necessary. The companies, however, it must be remembered, have had the hand-brake in use for many years, and it has been left to persons not specially connected with railway work to point out that the hand-brake is radically wrong—for, as every one knows, it is impossible to always avoid skidding with it. In view of that fact, and of the statement that the evil effects of skidding were well known a quarter of a century ago, it does not say much for the inventive skill of the profession that hand-brakes were not long ago improved off our trains. The explanation of the diminished retarding force when the wheels are skidded is most likely that given by Prof. Kennedy, though it might be worth while to study the question experimentally by means of heavy weights resting with a small surface on a metal rail. As long as wheels revolve, says Prof. Kennedy, the surface in contact with the brake is continually changing, so the tire does not become highly polished, but directly the wheels are skidded there is theoretically only a point, and practically only a very small surface, taking all the friction between the rail and the wheel. This surface must be almost instantaneously polished, and the wheel consequently slips along with the least friction possible between it and the rail; for, as is shown by the experiment, the friction increases as the velocity decreases. The paper has now, however, drawn attention to the subject, and it is to be hoped it will be worked out in a thoroughly scientific manner. Capt. Galton deserves thanks for what he has already done, and it is not too much to expect that the companies generally should afford facilities for carrying out further experiments.

THE discovery of an extremely simple and cheap means to protect houses from being struck by lightning has recently been announced in a French agricultural paper. This consists in the use of bundles of straw attached to sticks or broom-handles and placed on the roofs of houses in an upright position. The first trials of this simple apparatus were made at Tarbes—Hautes Pyrenées—by some intelligent agriculturists, and the results were so satisfactory that soon afterwards eighteen communes of the Tarbes district provided all their houses with these bundles of straw, and there have been no accidents from lightning since in the district—at least, so says *Nature*.

IRON AS A BUILDING MATERIAL.

From "The Architect."

USING a popular formula of speech, it is often said that iron is the material of the future. The fancy of the philosophic builder is supposed to run over a hundred instances in which the more commonplace substances used in construction are found wanting. Visions of what might have been if ingenuity had not been hampered in its enterprise by the conditions attaching to mere stone and brick, timber and boards, are supposed to overwhelm his mind. He finds rest in the contemplation of the Crystal Palace, the St. Pancras roof, the Britannia Bridge, the Vienna dome, perhaps the *Great Eastern*, the *Devastation*, and the *Thunderer*. "Ah, well!" he reflects, "iron is the material of the future; the time will come, although I shall not live to see it, when a gentleman will run his iron house down to his place in the country by rail in August, and up again to the Belgravia of the day in February; when balloons of No. 40 or 50 gauge sheet will travel daily between London and New York; and when a new St. Albert's Cathedral, in a central situation at Wimbledon, will be built of Professor Barff's best black oxidised." Professor Barry, for instance, of the Royal Academy, who officially might not have been expected to look so far ahead, is amongst others as enthusiastic upon this point as could be desired. The architecture of the world in the future can scarcely fail, he says, to be modified by our scientific knowledge of iron, which as a building material has been almost discovered by the present generation. From the Egyptians —to whom it is, of course, impossible not to allude—we have no doubt much to learn; from the Greeks also. But had the Romans known as much about iron as we do they would have been able to teach us something. The mediæval builders also would not have clung to their primitive arcuation if they had known about iron. In the present day architects are too considerate of the past; if they would but consent to let engineers help them in construction in exchange for similar assistance in decoration—in short, iron would then become the material of the future.

The Conference of Architects, which was held last week, seems to have dealt with iron, if nothing else, seriously. Professor Barff explained his system of creating upon the surface of this metal —as the weather does upon certain others, such as lead and zinc—a preservative oxide. Under the presidency of Mr. George Godwin a variety of fireproof inventors discoursed to each other upon the protection of iron from its inevitable destruction in great fires. Mr. Barlow, C.E., described at another meeting the construction of an iron roof recently designed by him; and thereupon Mr. E. M. Barry wound up the whole with the thoughtful reflections we have quoted. If nothing comes of all this, it cannot be said that architects have not at least, and at last, taken the subject into consideration.

But there are people of still more careful habits of thought, who will shake their heads, and say that nothing can come of it after all. Indeed, when Mr. Barlow, speaking incidentally of the great Tubular Bridge of Robert Stephenson, tells us of one thing being perfectly clear—that no such structure will ever be built again; and when Mr. Carroll, of "unpractical romantic Dublin," tells us how he and an engineer companion, as they traveled along it, shook in their shoes with a great fear lest the wonder of the world should shake itself and all that was within it forthwith into eternity, by reason of the "tons upon tons" of ruinous red rust shaken perpetually from its dreadful flanks; these authorities are indicating pretty clearly that the scientific mind is already being rapidly disillusioned, and that before long there will be no one left to believe in the perfectibility of iron buildings, unless it be such a one as a professor, whether of architecture or of chemistry, in the Royal Academy.

It is by no means a paradox to say that Nature does not undertake to supply man with building materials. He is permitted, no doubt, to hew stone from

the rock, and to fell timber in the forest, and it must be acknowledged that these accidental products have gone very far indeed to serve the builder's purposes; but the not unreasonable theory that the artificial objects of building must be taken to point to the use of correspondingly artificial materials is one that has in reality been exemplified from the most primitive ages—in the invention, for instance, of such an odd thing as brickwork; and when we are led in modern times to try what can be done with iron, it is the self-same principle that is manifesting itself—building is being driven by its own essential artificiality to seek artificial materials. In other words, reasoning upon the matter *a priori*, if not otherwise, we are entitled to say that Nature cannot be expected to provide to the architect and the engineer, more than to the machinist, anything beyond the crude components out of which he shall make for himself such materials as shall best serve his ends. But however this may be, it is plain enough that in this respect the line must be drawn somewhere which shall divide the practicable from the impracticable; and it is, perhaps, more than probable just now that that line must be taken to exclude iron in a very great measure from the list of true—that is, permanent—building materials, and to leave it almost entirely to mechanical engineering and other such manufacturing art as its more proper province. Such perfectly artificial materials, for instance, as brick, terra-cotta, artificial stone, concrete, cements and plasters, lead, glass, paint, and so on, answer the builder's artificial purpose admirably. There are, likewise, many appliances of building, akin to mechanical work, in which iron is almost as invaluable as it is to the mechanician generally. There are also certain incidents of building in which, for even structural features, iron comes to take the place of timber with excellent effect, as in columns and girders judiciously introduced. But here it would really seem as if we must stop for ever; crude as natural stone may be, iron can not take its place, and, fatal as may be the effect upon timber of the dilapidation of centuries, the case of iron as a substitute is much more serious within much shorter periods of time.

The employment of iron in ordinary building is to be fairly described as being altogether that of an equivalent for timber. The principles involved—those of the post and girder, the bent arch, the truss, and whatever else—are precisely those of timberwork, and a sheet-iron covering merely takes the place of boarding. Bolts and rivets represent screws and nails, and even the angle iron has its prototype in the work of the joiner. The only advantages derived from the use of the metal are in respect of strength and lightness, complexity of scientific design, and minute precision of calculation. Apart from these considerations, we might just as well even now be dependent exclusively upon our old-fashioned fir and oak—old-fashioned, no doubt, but still as far as ever from being obsolete. Where, then, is the great drawback in the use of ironwork? Why is it that it it has not during the last fifty years, since the invaluable article of poor Cort's invention—rolled iron—has become so intimately available and so cheap, acquired an absolute ascendancy over the timberwork which seems by its side so clumsy and unmanageable? The answer may be given in single word—Rust. Of all metals, perhaps this, the most useful in a thousand ways, is the worst to wear against the weather. Moisture in the simplest form is its deadliest enemy. Lead or zinc, for instance, as we have already hinted, when exposed to atmospheric action, becomes coated with an oxide of itself, which renders paint useless as a preservative; but iron, in forming its oxide in the same circumstances, develops a process of absolute disintegration, and falls rapidly to powder, and no preservative process yet known will protect it. Common painting, it has to be borne in mind, is simply the act of attaching to the surface of any more perishable material a coating of carbonate of lead as a material less perishable and easily renewed. Not merely oil paint, however, but the application of a coating of zinc, a much more scientific and successful invention, is scarcely of any permanent use in practice; and if we fail in protecting our ironwork from disastrous rust, we fail in making it really serviceable as a recognised building material. Not only the architect, but the engineer none the less, must ac-

knowledge this; and when the architect is obliged to discard iron in so great a measure, it becomes a question of time when the engineer also may have, however reluctantly, to regard it with universal anxiety.

Supposing that the general surface of the iron may, by the judicious application of some specially judicious coating, and its frequent renewal, be kept quite free from oxidation, this unfortunately does not help us after all. It is the peculiarity of ironwork that it is never at rest. It expands and contracts considerably under ordinary changes of temperature. It vibrates still more considerably under ordinary pressures. If, therefore, we are obliged to put it together by means of such a process as riveting—if, in other words, we have to make it up of small pieces pinned together—then are these considerations which at once appear with reference to rust. A thousand joints offer access to the microscopic influence of atmospheric moisture in a thousand places. A thousand pins—call them by what name we please—are in one way or another constantly moving under strain, however minute their movement. Nor is this all; for, in the very act of putting the work together at first, if any preservative had been previously applied to the surfaces that are now brought into contact under the force of the smith's hammer, it is only too plain that at the very weakest points of all the preservative has been abraded quite away, and the veriest nakedness of the metal exposed again to the most direct and rapid creation of rust. Not only oil paint, but what is called the galvanized coating of zinc, is obviously immediately rubbed off whenever a rivet is hammered, or even a bolt tightened by a wrench. What makes the case still worse is the circumstance that oxidation, when once begun, will insidiously continue to progress even under the preservative coating. It is easy, then, to see that, of all materials as yet employed in building, iron is in practice the most incapable of defence against a peculiarly disastrous decay produced by the most commonplace, most universal, most unavoidable, and most insidious process of attack. The invisible and motionless vapor of the air, which nourishes the world, is the inevitable and special destroyer of the mightiest substance manufactured by the ingenuity of man.

That these reflections are a serious check to the aspirations of building science it is needless to deny, but enough has been said to show even to the meanest capacity that, so far as it has yet gone, iron is emphatically not the material of the future.

THE BRITANNIA BRIDGE.

From "The Engineer."

At a recent meeting of one of the architectural societies it was gravely stated that the great bridge of Stephenson's was rusting away. The process of decay was progressing with alarming rapidity; consumption, in its worst form, had seized upon the noble structure; the disease was incurable, and its days were numbered. These statements publicly enunciated naturally somewhat alarmed outsiders, who began to entertain the notion that they might perhaps be correct, and that, at any moment, the Straits of Menai might engulph the Britannia Bridge and the Irish mail, passengers and all. We trust the protest of the engineer-in-chief of the London and North Western Railway, published in our daily contemporaries, has dissipated so absurd and unfounded an idea. It is just possible that it may have occurred to some one that since many old stone bridges over the Thames have disappeared, and Waterloo Bridge, upon excellent authority is shortly to do the same, that it was high time, upon the principle of fair play, that an iron bridge ought to begin, at any rate, to show some signs of decay.

The Britannia tubular bridge belongs to a particular class of structures of which we shall never see any more ex-

amples. As it is, that class has been reproduced, we believe, in only two instances; one of these is the Victoria Bridge at Montreal, and the other, a bridge of the same name in Australia. There can be very little doubt that the idea of the tubular form was either suggested to Stephenson, or if conceived upon other grounds, he was confirmed in the idea by the information he received from Fairbairn with respect to the strength of iron ships. An iron ship, allowing for the difference in form and other details of design, represented then as it does now a complete iron tube, if we regard the deck as constituting the upper boom. If, again we imagine the ship supported, as no doubt she often is, near each extremity upon the crests of two waves, she becomes an absolute tubular girder for the time being. It must not, however, be supposed that because we shall not construct any wrought iron bridges upon the model of the Britannia Bridge, that we thereby constitute any argument against its original merit, its present security, or its future durability. We are not likely to build any cast iron bridges in accordance with the design of Southwark Bridge; but that does not prevent that structure from possessing the largest span in cast iron in the world. The nearest approach to it, with the exception of the Sunderland Bridge over the Wear, are the seven arches of the bridge of Tarascon over the Rhone, which have a span of 203 feet each. It is now nearly thirty years since the Britannia tubes began doing their duty, and it is not so much a question whether they have suffered during that period from those causes which ultimately weaken and deteriorate every artificial structure, as whether the amount of deterioration is accurately known and provided for. Those who have read the letter of Mr. Baker, published in a daily contemporary not long since, will be assured that with respect to both these points, the condition of the Britannia Bridge is in every way as satisfactory as when the tubes were first erected.

Having touched upon the subject of the corrosion and consequent deterioration of iron bridges, it may be of interest to our readers to inquire generally a little further into the matter. As it is with timber, so it is with both cast and wrought iron. A great deal depends upon the quality of the material itself, and the medium which surrounds it. Some descriptions of timber will last, if wholly and constantly immersed in water, practically speaking, for ever. Timber piles have unquestionably been found perfectly sound after an immersion in water of over 500 years. The statement that the piles of Trajan's bridge were discovered perfectly sound after the lapse of sixteen centuries, must be received with caution. Other descriptions of timber will last a long time in a dry atmosphere, but not when exposed to damp; and very few indeed will stand exposure to alternate wetting and drying. Cast iron, again, has been found, in one locality, to be so soft after some years' immersion in salt water, as to be readily cut with a knife. In another locality of a similar nature, it has remained for fifteen years as sound as when first immersed. This case scarcely applies to the kind of deterioration under notice, which is limited more particularly to wrought iron.

The corrosion of wrought iron, to which structures in the position of the Britannia tubes are subjected, consists, practically, in the oxidation of the various bars and plates, and of the ironwork generally of which the tubes are built up. The oxidation takes the form of rust or scale, which sometimes falls off, and at others is removed at the periodical cleaning and repainting of the ironwork. Obviously, every successive formation and removal of this scale diminishes the original thickness of the iron, and it becomes a mere matter of time until that thickness is reduced to zero. The remedy, as regards maintaining the strength of a wrought iron bridge, clearly consists in either preventing the formation of the scale or allowing for it. No means have yet been discovered which will completely secure the first of these objects, although much may be done towards it. It is not difficult to carry out the latter plan. If the rate of oxidation for one, or any number of years, can be ascertained, even with approximate accuracy, the necessary extra allowance of material can be easily provided. It will first be requisite to determine what that rate is, more especially

as it varies with the material employed. If the medium be damp air, the relative oxidation of steel, wrought iron, and cast iron is about 1.12, 1.08, and 0.84. It has been inferred from experiments that the oxidation, or depth of corrosion of ironwork when exposed to clear sea-water, increases at the rate of 0.00215 inches of thickness per annum, which is equal to nearly $\frac{5.5}{2.5.0}$ inches in 100 years, or to $\frac{5.5}{1.2.5}$ inches in 200 years. There is not any plate in the Britannia tubes whose destruction would jeopardise the safety of the bridge which has a thickness less than $\frac{1}{2}$ inch or $\frac{6.4}{1.2.5}$, so that upon the assumption we have made, the tubes would, in about 232 years, be entirely corroded or rusted away. There is just one little saving clause in the case, which might add perhaps another fifty years or so to their existence—it is that the scale of oxide might adhere to the iron, and thus very considerably diminish the rate of oxidation of the remainder of the iron.

The Britannia Bridge is placed at an elevation of about a hundred feet above the sea level. It is, therefore, apparent that the supposition that the ironwork is exposed to the immediate action of sea water is not correct, and that the tenure of life assigned to it upon that supposition is too short. Let us consider the tubes, then, exposed solely to the action of rain or fresh water. Under these circumstances, and making the calculation from the same datum, the annual depth of corrosion of the iron will be 0.00035 inches, or at the rate of rather less than $\frac{3.5}{1.0.0.0}$ inches in 100 years, or $\frac{7}{1.2.5}$ inches in 200 years. The life of the tubes under these conditions would be about 1400 years. But this supposition is probably as much too favorable for the bridge as the former is unfavorable. The tubes, although not actually wetted by the salt water, are, nevertheless, acted upon to some extent by its saline qualities. They would be exposed to the action of rain, which would wash away the rust, and constantly expose new surfaces for oxidation. Under the most unfavorable circumstances the bridge would, however, last at least 100 years. Such a line of reasoning takes no account, however, of the conservative powers of paint, which, if of good quality, and applied with sufficient regularity to surfaces which would otherwise be denuded, may prolong the life of an iron structure almost indefinitely. Making all allowances, therefore, it is not too much to say that, with common care, the Britannia Bridge would last 150 years without any heavy repairs.

It is well known that the greatest possible skill and prevision were exercised in selecting the iron and executing the workmanship of the Britannia Bridge. At the same time, it is very possible that some parts of it are, either from greater exposure or other causes, more liable to corrosion than others, and might, therefore, be sooner deteriorated. In this case nothing is easier than to cut out the damaged and weakened plate and rivet on a fresh one. In fact, the whole bridge might be gradually reproduced piece by piece in this manner without affecting the integrity of the design or its practical efficiency. The parts of the structure most liable to corrosion are the outside plates composing the upper and lower booms and the sides, and these are precisely those which are the easiest to replace. The complicated and troublesome portion of the work lies in the ironwork of the top and bottom cells. A very recent examination has proved all the ironwork in these parts of the tubes to be in a perfectly sound and unimpaired condition. Experiment has established one more fact in connection with the corrosion of iron structures which is worth mentioning, as it bears immediately upon our subject. It is that iron when subjected to repeated vibration does not corrode with the same rapidity as when in a constantly quiescent state. The number of trains passing daily and nightly through the Britannia Bridge do not allow it much actual rest. If to these we add the expansion and contraction, and the influence of winds, slight although their effects are, we doubt if the tubes are ever in a state perfectly free from vibration. Wrought iron bridges are comparatively of too modern a date to afford any reliable information respecting their ultimate durability. It will require another fifty years before the problem will be in a fair way of being solved, and we may, therefore, be excused if we decline to say precisely how many hundred years the Britannia Bridge will last.

SOME PHENOMENA EXHIBITED BY THE COMPASS IN MINING SURVEYS.

By WILLIAM LINTERN.

From "Engineering."

The general opinion of the action of the magnetic needle used to be, and, I think, generally still is, that, unless diverted by purely local and accidental sources of attraction, and which are, therefore, removable, the needle will adjust itself parallel with the magnetic meridian of the place and time in all positions; and that, consequently, when free to move under such conditions, it will in a series of different positions maintain a true parallelism.

Several years ago, having occasion to make a survey of a certain colliery of considerably over a mile in length, and with particular accuracy for a definite purpose, I first made the survey with the needle, fixing it to the zero of the instrument each time, and working off the limb, and reading to minutes; I next made a check survey over the same lines without using the needle further than to get the magnetic bearing of the first line, so as to insure—as I supposed I should have—the same parallelism as before in the previous survey; after the first line I used the instrument simply as an angleometer by setting the limb with the precise previous reading back each time upon the back light, and I simply liberated the needle at each station for the purpose of observing its action under those circumstances; and, to my surprise, I soon found such a variation in the parallelism of the needle, as the work progressed, that I came to the conclusion that an error in manipulating the instrument had been committed; by re-observations of the lines I found this was not the case, and I determined to proceed again in the same way throughout the whole length of the survey—in all over 40 lines—and particularly to watch the action of the needle.

In the majority of the lines I found a marked variation of the needle bearing, and in scarcely two successive positions would it assume precisely the same parallelism; sometimes it varied in the aggregate of a number of lines to as much as 2° 30′ on one side of zero, then it would gradually return back again towards zero, and then progress to a considerable variation on the other side,—thus oscillating to and fro several times over the zero as the work progressed. The successive angles of the second survey were reduced on the base of the magnetic bearing of the first line, taken as before explained, and both surveys were carefully plotted off the same meridian line and position; and the result was that on comparing the two series of lines, although there was a *general* agreement in the direction of the corresponding parts of the surveys, there was yet a distinct minute difference, and such was the divergence as the laying down of the surveys progressed, that the final positions were 120 links apart; and, taking into account the fact that a straight line drawn from the initial to the final position or station measured 70 chains or thereabouts, the magnetic bearing of the first line of the angular survey, when compared with the average of the readings of the magnetic survey, showed that there was an error in one or the other equal to 59′.

Satisfied that the variations which I had here so carefully observed were not the result of what are generally called removable causes, peculiar to this particular colliery, I have from time to time over a number of years, and with different instruments, and under a variety of conditions both on the surface and in the mines, taken steps to observe the peculiarities of the working of the magnetic needle; and in the result I have found that a variation, more or less, is very general—more general indeed than an accurate parallelism is.

I will here give some examples to show this variation more forcibly.

Ex. 1.—In a heading crossing the pitch of the strata from one vein of coal to another (technically called a "cross-measures" heading), a straight line was carefully ranged out, and at nearly equal

distances apart, over a total length of about 60 yards, the instrument was set up five times in correct alignment, and the magnetic bearing of the lights purposely fixed at the two ends of the line were observed from each position; and the result was, that what is generally supposed would have been five similar readings, turned out to be as follows, viz., 174° 3′, 175° 21′, 174° 45′, 172° 30′, and 174° 40′, thus indicating a maximum variation equal to 2° 51′ in a line not more than 60 yards in length.

Ex. 2.—In a heading driven in a vein of coal 4 feet thick, and into and through a piece of faulty ground, consisting mainly of a mixture of rock and cliff, a line of about 60 yards in length was ranged out as before, and the instrument fixed first at that end of the line away from the "fault," and the light observed and read at the other end of the line within the faulty ground; seven other positions were then fixed upon in correct alignment in succession towards the other end, and the readings taken at each, and the result was the following series, viz., 36° 24′ 36° 20′, 37° 50′, 38° 15′, 39° 40′, 39° 10′, 38° 10′, and 37° 0′, in this case indicating a maximum variation equal to 3° 20′.

The line of the "fault" crossing the alignment of the several positions was an acute angle, and the sixth reading was about in the line of its crossing, and the seventh and eighth readings were within the fault.

By referring to the several readings it will be observed that there was an increasing divergence in the same direction (to the right) in approaching the fault, and that after entering the fault there was a sudden twist back again in the contrary direction.

Ex. 3.—A series of magnetic bearings was taken in an engine plane underground, which was driven quite straight from end to end, and the bearings were taken previously to the setting up of the ordinary fixtures of an engine plane, which usually interfere with surveying operations prejudicially; and over a length of about 330 yards the following readings were accurately observed, viz., 346° 55′, 345° 0′, 346° 42′, 346° 15′, 345° 0′, 346° 30′, 346° 9′, 345° 48′, and 347° 3′, thus showing a maximum difference equal to 2° 3′.

Repeated trials on carefully ranged out surface lines do not indicate the prevalence of so great a variation of magnetic readings as underground lines, but even these show frequently a marked variation. The following examples are given as evidence of this:

Ex. 4.—On a surface line of about thirty chains in length the instrument was set up five times in correct alignment, and observations taken, and in this particular example the readings at each position corresponded precisely with all the others.

From one end of the previous line, and almost at right-angles with it, another line of about twenty.four chains was ranged out in the same manner as before, and the following series of readings taken:

Ex. 5.—54° 58′, 54° 51′, 54° 44′ and 54° 58′; these therefore almost indicate a much less variation than in the lines underground.

Ex. 6.—In a long carefully ranged base line of a surface survey of considerable extent several observations were taken as at other times, and the following were among the readings taken down, viz.: 114° 41′, 114° 41′, 115° 7′, 115° 21′, showing in these a maximum variation of 40′. This variation, although it does not look so formidable as some of the previous ones given, yet, when analyzed, it represents something serious; for if viewed in reference to the length of that section of the line, at the extremities of which the instrument was set up and the readings taken—in one case 40 chains, and in another 26.45 chains—we shall find that in the former case the *twist of position* due to the variation (and consequently the error that might have been thus imported into the work), is equal to 46.5 links, and in the other case it is equal to 30.7 links; and this is a consequence scarcely to be neglected or overlooked.

The foregoing examples, confirmed by many other observations made from time to time, plainly indicate that the magnetic needle does not—even when used on the earth's surface—maintain generally an accurate parallelism, and that when used in underground operations the variations are generally much more marked.

This subject has, of course, a primary

bearing upon the use of the magnetic needle in surveying operations; but it has often occurred to me that this effect of the ceaseless operation of magnetic forces may not be, and most probably is not, the sole and only consequence of manifestation to us.

What the intrinsic change really is which a piece of steel undergoes in the process of being magnetized, and converted into a magnetic needle, I have never been able to understand to my own satisfaction; but my observations lead me to suppose that whatever the *internal* change may be upon the steel, it results *externally* in imparting to the needle the power to *conform to the direction* of the current of magnetic force passing around it at the moment, and in the position in which it is being used.

I have often observed on different occasions that the needle seems to be more deflected from its true parallelism when used in close proximity to faulty and disturbed ground, and also when used in headings passing through such varying ground as is met with in what is technically known as "crossing the measures," than in ground of a more uniform nature, whether it be an ironstone mine or a coal mine; and the conclusion I arrive at in view of these experiences and circumstances is, that the needle deflections represent the deflections of the passing current of magnetism in the surrounding strata, and that these deflections of the current are again the result of the varying powers of conduction possessed by the varying strata of the earth; that, in fact, as water turns aside from the more confined parts of its channel to that which affords it the freest passage, so does the magnetic current get slightly deflected, first to one side, and then to the other, in its passage through the strata, the best conductor conveying the greater quantity; and when this superior conductor comes to an abrupt end, or becomes distorted or disturbed, either from a "fault," or from some other cause, the current becomes more or less deflected, and the magnetic needle used in close proximity to such a position, or locality, would also in its turn become deflected in sympathy with the current.

But I conceive that there is a great probability that this same subtle power frequently operates to the causing of other consequences, which are often not a little perplexing to account for, and to understand.

In that state of the weather when the atmosphere is highly charged with electricity, and heavy storms of rain are frequent, we often experience the springing up of a sudden wind, which, leading in the van, as it were, as well as bringing up the rear of the disturbed elements, blows furiously for a while until the rain has ceased, when the wind again gradually subsides into a perfect calm. To my mind the theory that winds are caused by the rarefaction of the atmosphere in certain localities, to which the air rushes to restore the equilibrium—thus causing winds—utterly fails to afford a sufficient and satisfactory explanation of the occurrence of these suddenly springing up and as suddenly subsiding winds, carrying, as they seem to do, a furious storm of rain, or hail, or snow in their bosom.

But whatever may be the intrinsic nature of the force put into operation, whether electricity striking out abnormally (if such an expression may be permitted) in a deflected line or otherwise, it is certain that the *vis viva* of the power thus set in motion represents an enormous aggregate of force, as the destruction sometimes wrought by a small portion of it sufficiently attests.

Disasters, sudden and startling, sometimes occur in collieries from the explosion of gas; and the only explanation frequently possible is, that a sudden outburst of gas has occurred and overpowered the ventilation, and that from a defective lamp, or from an unprotected light, the gas exploded; and we not unfrequently find the sudden outburst of gas explained and accounted for by saying *that a "fall of roof"* took place. Now I am strongly of opinion that where these two things are found to have occurred together, they are not necessarily, nor obviously, cause and effect in the order named, but that, much more probably, if they are not two effects of the same cause, the fall of roof is a consequence of the explosion.

When a vein of coal has been extracted from its position in the strata over a considerable area, the roof, or the floor, or both, will be sure, sooner or

later, depending upon their natural and also their relative strength, to show a tendency to close up the space from which the vein of coal has been extracted; if the strata in which the coal lies is of a friable nature, and readily breaks up, the large interstices resulting from its closing up the space formerly occupied by the coal will necessarily be much more ramified throughout the broken strata, but will not form one or two large chambers; if, on the other hand, the strata is of a more tenacious nature, and will bear a very considerable subsidence or elevation before it will break up, then a chamber more or less large, either in the back of the subsidence or beneath the upheaval, or both, will necessarily be the result.

These ramifying intervening spaces as in the first case, or the more extensive chambers as in the second case, will not be in vacuum, but will become filled with the air or gas, or a mixture of both, so fast as they are formed; if the strata give off carburetted hydrogen gas, then it may be taken for certain that an explosive mixture will very soon, by reason of the operation of the law of diffusion of gases, occupy the whole of the spaces and chambers so formed.

Let us now assume the occurrence of quickened activity in the earth-currents in our latitudes as are so frequently, though more forcibly, experienced in some other parts of the world (and which, when they are atmospheric, *we* have such sensible and frequent experience of), and we shall not be assuming too much if we credit those earth-currents with a very largely increased *vis viva* under such circumstances; let, then, such chambers as are mentioned above, and filled with an explosive mixture of gas, lie in the path of such earth-currents, and their *vis viva* will immediately tell upon a body so imponderable, and such an impulse would be imparted to it as would immediately drive a considerable portion of it through the joints of the ground communicating with the coal workings, and if a naked light or a defective lamp should be within its reach an explosion would be certain to ensue; and once a portion of it became ignited, the explosion would extend to wherever the train of the gas in the requisite mixed proportions extended, even to the partially emptied chambers of the roof or floor; and where such happens the strongest roof must give way and be blown down, seeing that the expansive energy of such gas immediately after explosion is about five atmospheres, or 75 lbs. per square inch. And hence I consider it much more probable that the "fall of roof" is the result of the explosion instead of its being an antecedent consequence of it, and contributing in that sense to bring it about.

A friable roof and floor may, also, in this view, from the fact of its more readily breaking up, and thus preventing the accumulation of so large a lodgment of gas in a single chamber, and also by facilitating the more continuous drainage of the gas into the passing air of the mines, render the colliery far less subject to sudden outbursts of explosive gas than a mine with a much stronger and more tenacious surrounding strata would be; and thus, on the whole, the former would be more safe from that class of accident than the latter.

I cannot deny of course that some of the opinions I have expressed here, and some of the conclusions I have drawn from them, may possibly be characterized as being insufficiently supported by my premises; the existence, however, of such magnetic variations as I have here demonstrated, and the known fact of the existence of those powerful earth-currents that make their presence and power felt so forcibly in some other parts of the world; and also remembering those atmospheric disturbances which are so universally felt at times in all parts of the world—these appear to me to justify such a train of reasoning as that I have here entered into; and if what I have here written should lead to investigations by abler hands than mine, from which good may ensue, and our knowledge of these things become more extended, I shall be as much gratified as any one else can be.

In an interesting paper lately read at a meeting of the Royal Society, on "Experimental Researches on the Temperature of the Head," Dr. Lambard showed that mental activity will at once raise the temperature of the head, and that merely to excite the attention has the same effect in a less degree.

CLEOPATRA'S NEEDLE AND ITS WORKMEN.

From "The Builder."

We have had the opportunity of carefully inspecting the now familiar Cleopatra's Needle. It has been exposed partially to public view, and a little at least can be readily seen from the Embankment. We call attention to it now, and while it is in its present *bonâ-fide* state, as it is while in that state that such a monument is really and truly interesting to the lover of past art and methods of workmanship. So much indeed,—may we not say everything?—round and about us of our own antiquities has changed and been modernized, that a glance,—as here,—at a genuine "antiquity," in its rough and time-worn state, is quite a novelty,—a something really strange to see, and leaving an impression not to be got at in any other way. The preparatory work, it may be mentioned, of providing a pedestal for it to stand on is rapidly progressing; and it is earnestly to be hoped that this too elaborate pedestal will not dwarf, and make quite secondary, the monolith itself. We here propose to make note of it as it now is, and while it tells so simply its own story, and to call attention to the workman's part in the granite cutting and carving of it, and which, to say truth, needs no added work to make it attractive.

So many descriptions and accounts of this "Needle" have been already given that it must needs be familiar to most, but there are yet one or two things connected with it which have been hardly noticed; but they are vital elements in the matter notwithstanding. A word or two, then, may at the present juncture prove useful. We are told in an authoritative book on Egyptian history and antiquities, that of all works of Egyptian art in simplicity of form—we ask note of this—colossal size, and unity and beauty of sculptured decoration, none can be put in comparison with the obelisks. The Cæsars of Rome vied with the Pharaohs of Egypt in their admiration of the obelisks, but it is not said that these same obelisks were put up in the places where they were found, because they were pretty to look at, or as attractive monuments; they were, indeed, and simply, pieces of the temple furniture, just as much so as any item of church furniture is a thing of use and necessity in a church of to-day. Obelisks never stood alone and isolated as this one on the Thames Embankment is to stand, but always in pairs, and immediately in front of some building or pylon; so that in approaching them, and getting sight of them, they were seen detailed against the huge mass of walling near which they stood, and were thus seen at their very best, their long shadows being all but a part of them.

The use and origin of the obelisk is yet as debateable as ever, and why these were placed at the entrance of the great temples, and always in pairs, is not apparent, and whether or no any pause or ceremony took place on the occasion of the long procession when passing between them into the Temple is not known, and can be only conjectured. All that we do know is, and of this we may feel quite sure, that they were not cut out from the quarry, and brought to their places, at such a vast cost of labor, for the mere sake of putting a something in the places where they stood, but that they had a peculiar and highly significant meaning, and were, indeed, essential parts of the Temple apparatus, whatever that might have been. It may be that, could we be quite sure of the hieroglyphic reading, this would be explained. Objects so conspicuous and so striking must need have been highly symbolical in purport, and must have been as open books to be read in the passing by them. This absence of a building, of which the obelisk formed a part, and the fact of the ever-present but mysterious writing on it, would startle the old Egyptian builders and workmen not a little, could they but return for a brief moment, to look at their work on our river Embankment.

But our object at present is not to go into the history, and even uses, of the obelisk, but to make note of its artistic

character, and of the cutting of the hieroglyphics on its huge surface. We have examined this with some attention, and would recommend the study of it to our stone-carvers. The actual material out of which this monolith is cut is hard granite, and right good tools and skillful hands only could have made impression on it. This granite-cutting is remarkable in many ways. It is not simply the carving out of the hard and intractable substance the forms we see, but the indications of manner which are to be noted in the doing of it. Large, and apparently rough, as the granite-cutting is, there is the constant presence of the artist workman to be seen in it. The surfaces are not all of a uniformly dull flatness, as such work would now be made, and as it is done when "lettering" is cut out of stone; but a thorough knowledge of the form and even life of the object represented is here, when such object admits of it. We would here ask the attention of those who have to do with such specimens of the workmanship of so long a bygone day to note this, so that no attempt whatever may be made at "re-cutting," or mending, or "restoring," as it would be called, of the work, or even repolishing it. If this be done, all the antique life of work goes. We hear that this is under consideration, but if so, before it is done, may we suggest casts of the hieroglyphics, and thus that, at least, a true record be preserved of them.

These hieroglyphics should be studied while the obelisk is where it now is, on a level with the eye. One thing, by the way, little as we know about the matter, was intended by those who erected obelisks, and that was that they should be as ever-open books, to be readily and easily read, they always standing on a low block of granite, so as to admit of this. The letters were close to the eye as could be, and even when near the top of the monolith were so large, and so deeply incised, that they could be readily read from top to bottom. Indeed, the longer this magnificent granite cutting is looked at, the more do you wonder at it, and at the skill with which it is done. In the clear sunlight of Egypt these hieroglyphs show themselves with an almost startling precision and distinctness. The old Roman was justly proud of his lettering on his buildings, and right well he did it, but it quite pales before such works as this, where the *forms* even admit of vitality in the rendering of them. Again, then, may we express a hope that they will not be tampered with, but left as the antique carvers cut them, and no attempt made to "polish" or recut them, or, indeed, in any other way to destroy or mar their individuality and antique expression.

We are here looking at this huge monolith as a specimen of the work that in its time was done in Egypt, and we cannot but wonder at the power of such work, when contrasted with what is now possible. Compare the mechanical appliances *then* and *now*, and well may we wonder at the skill and patience of the old Egyptian quarrymen and granite-cutters, who managed to subdue even this huge mass, and to cut it out of its natural bed, and to afterwards move it into its place. Nothing, indeed, would seem to have been too huge for the Egyptian workmen; blocks, however large and weighty, were quarried and moved long distances, and then set up with an ease and skill which might appal even our mechanical and steam-aided powers. Indeed, we hardly know which to wonder at most, the power displayed by the old workmen in the cutting out and the moving of such huge masses of so hard and solid material, or at the artistic skill and feeling afterward displayed in the "ornamenting" of them. We have much to learn even in these advanced days, and but few able to doubt it; but if any do so, why here is a proof in point, and he who runs may here read.

We do not intend just now to say a word on the pedestal, but would remind lovers of genuine antiquity that those who designed this monolith never dreamed of anything of the sort threatened!

It is impossible to make note, however slightly, of this really magnificent example of the skill and artistic power of the working artists of Egypt without an earnest hope that no attempt will be made to add to it anything that can be avoided.

It may here be of interest to mention that an Arab writer, in the twelfth century, notes that the obelisks had even in his day "copper caps" on their tops;

but these without doubt, he hints, were after-additions by those who had conquered the country. Our object now should, as we think, be to preserve this monument as an *Egyptian* antique, and as one purely and solely Egyptian, and thus to see it, as they of Egypt of old saw it, in all its simplicity and harmony of outline and strength of granite cutting. An obelisk is in itself so simple an object that it is impossible to add to it without, at the same time, taking away from it. Like a Stonehenge block, it can not be added to without injury.

HOW IT IS TO BE ERECTED.

The cylinder and its contents having been floated some three or four weeks ago over the temporary gridiron made to receive it on the up or Westminster side of the Adelphi Stairs, was, before being allowed to permanently rest on the gridiron, canted over on one side by the simple expedient of shifting the ballast. As canted over, the bottom of the vessel faced the Victoria Embankment, while the upper or deck side faced the Surrey shore. The vessel was canted over in order that that side of the monolith which is least "weathered," or, in other words, which retains the most sharply-cut hieroglyphics, should be parallel with and face the Embankment roadway. The side which will face the river is the most weathered of all, the remaining two sides, which will be at right angles to the Embankment roadway, being not so much worn. The vessel having been canted over, the first thing to be done was to begin pulling it to pieces. Nearly all the iron plates were removed, the ribs remaining intact, and the obelisk, wedged up from the gridiron, remained submerged at high tide. During low tide the obelisk, which has its point or pyramidion in the direction of Waterloo Bridge, has been slowly moved forward by means of hydraulic jacks, until, at the time of writing, the obelisk has emerged, point foremost, a considerable distance out of its iron shell, the apex nearly touching the stairs on the up or Westminster side. The next operation will be to raise the obelisk bodily to a height sufficient to clear, and to allow of its being traversed partially over, the landing between the two flights of stairs. When the obelisk has been centrally placed over this landing, it will be again raised to a height just sufficient to clear the two masses of granite (part of the Embankment structure) which will flank the obelisk when erected, and which masses it is proposed to surmount with sphinxes. Having attained this height, it will be moved laterally towards the Embankment roadway until it lies across the center of each of the flanking masses or pedestals of granite referred to. The obelisk will be moved in all cases by means of hydraulic jacks, and carefully "packed" as the work proceeds, so as to prevent undue strains upon it. The obelisk having been got into the position indicated, *i.e.*, lying horizontally across the spot upon which it will stand, will be cased in its central portion with a wrought-iron jacket, about twenty feet long, and riveted at the angles. This jacket will be made to fit pretty tightly by means of wedges of wood, and in order to prevent the stone from slipping out of this jacket a wrought-iron strap will be carried round from side to side under the foot of the obelisk. This jacket, which will weigh about 16 tons (making, with the obelisk, which weighs about 186 tons, a total of about 200 tons), will be fitted with strong projections or trunnions on the two sides facing the Embankment roadway and the river respectively, and these trunnions will rest upon two specially-made wrought-iron girders lying parallel with the obelisk itself. Each of these girders will be raised at each end by means of a hydraulic jack, and will work in and be guided by the recesses left in each of the four main uprights of the specially-designed scaffolding which will then have to be erected. Roughly speaking, these four uprights will form the corner boundaries of an oblong space 17 feet by 8 feet 6 inches, the two longer sides being parallel with the obelisk and spanned by the girders before mentioned, and the obelisk projecting for about a third of its length beyond each of the shorter sides of the imaginary oblong described by the four uprights. These uprights will be about fifty feet high, and will each consist of six "sticks" of timber, twelve inches square, arranged and bolted together three and three, parallel with the obelisk, with a space nineteen inches wide between each six for the ends of the girders to work in.

These uprights will, of course, be thoroughly braced together and stayed and strengthed by raking struts, &c. Each end of each girder will be simultaneously raised and "packed," until the girders, supporting the obelisk in a horizontal position by means of the trunnions of the iron jacket before described, shall have attained a sufficient height to allow of the whole mass being swung round on its trunnions, so that its base shall be but a short distance higher than the pedestal prepared for it, when, all being right, it will be gently lowered to its position. The pedestal, we may say, will rest on a foundation of Portland cement concrete, carried down to a depth of forty feet to the London clay. This part of the work has been executed by the Metropolitan Board of Works, for and at the cost of Mr. Dixon. The pedestal itself will be of hard bricks, set in Portland cement, and faced with blocks of gray granite (the same as that used for the Embankment wall) of considerable size. Of this pedestal a portion has been already erected, but the remainder will have to be built up after the obelisk has been raised, by the means described, above the highest course of the pedestal. A shallow groove will be provided on the top, in order to allow of the removal of the wrought-iron strap, already mentioned. Although the four corners of the lower part of the obelisk are very much abraded, there still remain about twenty-four superficial feet of flat surface at the bottom, and this extent of bearing surface will, it is believed, be fully sufficient to insure stability.

We believe that nothing is definitely decided as to the proposed sphinxes; but we may note that, in the "Visitor's Book," a gentleman has put on record the substance of a conversation he had with the late Mr. Joseph Bonomi, who expressed the opinion that, if sphinxes are to flank the obelisk, they should be of a date coeval with that of the obelisk itself. Mr. Bonomi only knew of two such sphinxes—one in the National Collection at Paris, and another in the Duke of Northumberland's collection at Alnwick—and he suggested that one of these should be adopted as the model of those which it is proposed to place in juxtaposition with the obelisk.

The work of getting the obelisk into position must necessarily proceed slowly. It is hoped, however, that the work will be safely effected by the end of August.

PROBLEM FOR ROLLING STOCK AND RAILWAY BUILDERS.

From "Iron."

Our English railway system is, beyond question, the most complete that exists. Nowhere else are such facilities enjoyed for reaching any desired point, and in the matter of high speed we lead by great lengths. Still we are far off perfection, and, indeed, in many minor respects our Continental and transatlantic neighbors excel us. One of these is the attention paid to the comfort of passengers; another, the better training in courteous bearing of officials; and others will readily suggest themselves to any who have had opportunities of instituting comparisons. One drawback to railway journeys in England is the swinging from side to side of the carriages. This is not a defect peculiar to us. It is no more guarded against across the Channel or in the United States than here. But in England we suffer more from the annoyance, because express riding is popular; and the measure of carriage oscillation much depends on the velocity of travel. The swinging and jerking incident to a ride of a hundred miles or so in an express train enervate and distress travelers, and, whatever the demands upon them, effectually bar the weak or invalid from so voyaging. The drawbacks are so manifest as to make it not a little remarkable that builders of permanent way and of rolling stock have not long since devised means to remedy them. The "Bogie" principle was evolved to meet the difficulty, and has contributed fairly to that end, we believe; but even that—and it can only be re-

garded as much less than what may be accomplished—has not been taken kindly to by railway corporations. The Midland is the only large company which has even partially adopted it. Probably the lack of remedy is traceable to absence of demand. We grumble at inconveniences long before we clamor, and it is only clamor that can wring concessions from railway owners, whom we are pleased to regard as the servants of the public, but who treat the public as farmers do their turnips—make as much out of them as they can with the least outlay. *Sotto voce* protest has now, however, ended, and agitation has begun. It is a singular fact that during the whole fifty years since railways were first introduced there has been no improvement in the wheel and axle arrangement, and the rigid fixture of the wheels now is just the same as Mr. George Stephenson adopted, and, indeed, found adopted when as a boy he saw them at work in the collieries of Durham. Two wheels are practically welded to a bar of iron, and neither of them can move without the other, so that in passing over a curved line of railway which has two rails of different lengths one of them must travel over a longer space than the other. In order to modify the natural action of these opposing conditions, the outer, or longer rail, is "banked up," and thus the perpendicular line of the load is changed, and the "grind" is produced by the flange rubbing against the rail; and it is owing to this action that so many train accidents happen of vehicles leaving the line. One of the wheels must "skid" more or less, and friction is thereby very much increased, the "wear and tear" of both the wheels and the permanent way is largely augmented, and so is the danger. An interesting correspondence is now going forward in *The Times* touching this matter. It was initiated by Mr. James Howard, who having, during two journeys to the Paris Exhibition, been keenly annoyed, was prompted to ask, "Have railway companies in England kept pace with the general advance?" Replying to his own query, he says: "If this question were to be answered from the experience gained upon the South-Eastern, and London, Chatham and Dover lines, it would, I think, have to be answered in the negative: on the contrary, if answered from experience of the Midland Railway—upon which I reside, and upon which many improvements have been adopted—it would, unquestionably, be answered in the affirmative. About a month ago I came to Paris, and chose the London, Chatham and Dover line, but owing to the oscillation of the carriage being so violent and alarming to myself and fellow passengers, I determined to try the South-Eastern route, and left London by the 9.25 p.m. train for Folkestone. Bad as was the former line, portions of the South-Eastern if anything were worse; the oscillation was so violent just before reaching Sevenoaks that, upon the train pulling up at that station, I left my carriage to speak to the guard. Upon saying to him there would be accident before long unless some improvements were made in the road we had just passed over, he remarked that for such high speeds this portion was bad. I do not want to endanger the lives of such valuable public servants as Colonel Tyler or Colonel Rich, but am persuaded if either were to take a trip on these two lines at express speed he would come to the conclusion that improvements were imperatively called for. I hope to see, at no distant date, engines and carriages upon the 'Bogie' principle universally adopted, as well as simultaneous, automatic brakes; they work admirably on the Midland. Time of course must be allowed for the wearing out or conversion of the existing rolling stock, but in respect of permanent ways, surely railway companies are bound by every moral consideration to maintain them in the highest possible condition; to alarm their passengers in the way I have described through failure so to maintain them is, to say the least, unpardonable. To feel that your carriage, being propelled at forty or fifty miles an hour, cannot keep the rails with so much swaying and bumping is a trial even those with the strongest nerves do not care to have repeated." Mr. Francis W. Dean, tutor in engineering at Harvard University, U. S. A., has also taken part in the correspondence. He says he has noticed on nearly every railway he has traveled on in Great Britain the same defects of which Mr. Howard makes complaint and endorses what he says touching the value

of the "Bogie" system as a remedy. This system, he adds, has further to recommend it the fact that it prevents the grinding of the flanges on the rails. In support of the latter proposition he writes:

"Although I have had an opinion upon this matter for an indefinite time, at York, the other evening, I became convinced that the amount of the grinding is not over-estimated by advocates of the Bogie system. While waiting at the station in that place, I heard squeaking between the flanges and 'metals,' which far exceeded anything that I had ever anticipated. The locomotives were noble specimens, and belonged chiefly, if not wholly, to the North-Eastern Company. As the Australian commissioner, Mr. Higinbotham, in his report of the railways of the world, has substantially remarked, such locomotives and carriages would hardly keep on the rails on less perfectly permanent ways than those in Great Britain. I may remark that I have traveled in both the Bogie and common carriages of the Midland Company, and found the difference very striking." The gravity of the defect animadverted on is palpable and the necessity for removing it obvious: and there are few save railroad proprietors who will not agree that should the adoption of the "Bogie," or any other remedy, ensue from the correspondence, a service will have been done the public by Mr. Howard and those who have with him participated in it.

STEEL PLATES AND RIVETED JOINTS.

From "Engineering."

A CIRCUMSTANCE connected with the greater ductility of soft steel compared with that of iron plates, which appears to us to require consideration, is the effect of this greater ductility upon the crippling strength of the plate, and consequently upon the proper proportions of the riveted joint. The softer and more ductile the plates the more liable is the material at that side of the hole that bears the stress to be crushed or crippled by the rivet bearing against it.

With iron plates and iron rivets, in order that the tearing, shearing, and bearing resistances may be theoretically equal in single riveted lap joints, if we take the thickness of the plate as unity, and assume the tearing stress to be equally distributed over the section of the plate between the holes, and the plate to receive no damage by punching the thickness of the plate, mean diameter of hole and pitch of rivets will be represented by the numbers 1, 2.6, and 7.6, the efficiency of the joint or the ratio of the strength of the joint to that of the solid plate being 0.66. As the diameter of the rivet holes in $\frac{1}{4}$ inch plates seldom in practice exceeds twice the thickness, and the pitch $4\frac{1}{4}$ times the thickness of the plate, it is evident there is an excess of bearing strength over both the tearing and shearing strength in $\frac{1}{4}$ inch plates with the usual proportions of joint, and this excess increases with the thickness of the plates, taking the diameter and pitch of rivets generally used.

With double-riveted lap joints taking the thickness of the plate as unity, we should have the thickness of plate, diameter of hole, and pitch of rivet represented by 1, 2.6, and 12.75. In practice, the pitch in $\frac{1}{4}$ inch plates double riveted seldom exceeds seven times the thickness, and three and a half times the thickness in 1 inch plates, so that the excess of the bearing over the tearing strength is even greater than in single riveting, the excess over the shearing strength remaining the same.

In reducing the thickness of plate when substituting steel for iron plates by the amount allowed by the excess of tenacity of the former over that of the latter, or, say, by 25 per cent. if we retain the same pitch and diameter of rivets, we shall maintain the same proportion of tensile and shearing strength in the plates and rivets, neglecting, for the present, in the case of lap joints the increase in the proportion of strength due to the stress being less out of line at

the overlap of the thinner plates of steel. The bearing surface of the plate will, however, be reduced by 25 per cent. If the resistance of soft steel to crippling were greater than that of iron, in the same proportion that the tenacity is greater, the reduction of bearing surface would be compensated for by the greater resistance to crippling. As, however, the ductility of soft steel is considerably greater than that of ordinary iron plates, it is extremely probable that the resistance to crushing is less. The resistance to crippling no doubt varies widely in different qualities of iron plate, but comparatively little is known of this resistance in iron and still less of that in steel plates. From the results of the few experiments that have been made with a view to ascertain its value for iron it is usually taken at twice the tensile strength of ordinary boiler plates.

If we take the tenacity of steel as being one-third greater than that of iron, which allows a reduction in thickness of 25 per cent. only, and assume the resistance to crushing as being 25 per cent. less, in order to compare the proportions of joint for equal tearing, shearing, and bearing resistance, we shall have, using the same mode of comparison as above, 1, 2, and 4.4 representing the thickness of plate, diameter and pitch of rivets for single-riveted lap joints in steel, and 1, 2, and 6.8 for double riveting, giving an efficiency of .54 and .70 respectively. These theoretical proportions of joints are much nearer what is used in practice than is the case with iron plates. In replacing $\frac{1}{2}$-inch iron plates with $\frac{3}{8}$-inch steel plates, and using $\frac{3}{4}$ inch rivets at $1\frac{5}{8}$-inch centres for single and $2\frac{1}{2}$-inch centres for double riveting, we shall have a joint with the tearing, shearing, and bearing resistances all equal. In replacing a 1-inch iron plate by a $\frac{3}{4}$-inch steel plate, we should require $1\frac{1}{2}$-inch iron rivets at $3\frac{1}{4}$ inch centres for single and at 5-inch centres for double riveting in order to have a theoretically proportioned joint, and by using $1\frac{1}{4}$-inch rivets at $2\frac{1}{2}$-inch and $3\frac{1}{8}$-inch centres respectively for single and double riveting, it is evident we shall have an excess of bearing resistance over that for tearing and shearing.

In the report of Lloyd's Registry Committee on steel for boiler making it is stated that in consequence of the crippling of the material behind the rivets in some experiments, it appears that a greater proportion of bearing surface is required with steel than with iron. Unfortunately the dimensions of the joint that thus failed are not given.

There are two ways of bringing up the bearing surface, (1) by increasing the diameter of the rivets, and (2) by increasing the number of rivets. By increasing the diameter of rivets and maintaining the same pitch, we diminish the efficiency of the joint, and if we attempt to increase the pitch in order to maintain this efficiency, we neutralize the very advantage sought in increasing the diameter of rivets. It must not, however, be forgotten that by increasing the diameter of rivets without altering the pitch, we may increase the proportion of bearing surface by a much greater amount than we reduce the proportion of tearing section. For instance, by altering 1-inch rivets at $3\frac{1}{4}$-inch centres to $1\frac{1}{2}$-inch rivets, we increase the bearing surface by 50 per cent. whilst we reduce the shearing section 20 per cent. only. In all cases it must be a question whether the increased bearing strength obtained by increasing the diameter of rivets is wisely bought at the expense of the efficiency of the joint. Whether we maintain the same pitch or not, we give a preponderating shearing strength to the rivets, by increasing their diameter beyond the usual practice for iron plates.

In seeking to obtain additional bearing surface by increasing the number of rivets in the same line and reducing their diameter, we reduce the tearing strength to the same extent as by increasing the diameter and maintaining the number of rivets. In this case we injure the plate more by punching, but the stress will be more evenly distributed over the plate, and we get a joint that is more easily made and kept tight if the rivets are not made unduly small. Here again the proportion of tearing section is not so rapidly reduced as that of the bearing surface is increased. Suppose we replace 1-inch rivet holes at $3\frac{1}{2}$-inch centres by $\frac{3}{4}$-inch rivets at $1\frac{3}{4}$-inch centres, the bearing surface will be increased 50 per cent. and the tearing section diminished 20 per cent., the shearing section being increased about 12 per cent.; or by using

¾-inch holes at 2⅛-inch centres, the bearing surface will be increased 16.6 per cent., whilst the tearing section will be reduced 6.6 per cent. only. When additional bearing surface is actually required, it is best obtained by making an additional row of rivets in the joint.

The carrying out of the recommendation to increase the diameter of rivets when substituting steel for iron plates may easily be pushed too far, and counteract some of the benefit we should expect to derive from the superior ductility of steel. If all rivet holes were drilled fair with the plates in position and close together, and if every hole were filled by its rivet to make a perfect job, in which each rivet takes its share of the stress distributed over the length of the joint, it would even in this case be scarcely advisable to proportion the joint so as to bring the crippling strength up to the tearing strength of the plate, for it is much better that the holes should elongate by crippling under severe stress, such as that caused by unequal and sudden contraction, and give warning by leakage, which might not require the renewal of the plates to render the boiler serviceable, than that attention should be drawn to the presence of the straining by the fracture of the plate from hole to hole, which is always a serious if not dangerous defect requiring partial or complete renewal of the plate, and which may occur without giving warning, through the crippling strength of the plate being kept too high.

When we increase the size of the rivets, we increase the bearing surface only directly as the diameter of the rivets, but the shearing strength as the square of the diameter. We should therefore increase the pitch in proportion to the square of the diameter, assuming of course that we are dealing with a joint well preportioned in the first instance, in which the plates between the holes should have a margin of tensile strength over the shearing strength of the rivets, since the plates are liable to become reduced in strength by punching and wasting, whereas the portion of the rivet between heads being protected does not become so much reduced. When the joint is not so proportioned, the less are we justified in still further giving a preponderance of strength to the rivet already too large. The greater the pitch of rivets the more is the strain concentrated at the sides of the holes, and consequently the greater is the tendency of the plate to be broken piecemeal and the breaking strength to be thereby reduced. Hence increasing the size of the rivets and attempting to maintain the efficiency of the joint is tantamount to increasing the brittleness of the plate, and by injudiciously proportioning a joint we may to some extent at least neutralize the advantages expected to be gained by annealing and using a ductile material.

One very important point should not be lost sight of in proportioning a joint, and this is that it is far more difficult to make a good repair job with large rivets than with small ones, especially in inaccessible situations, and where the pitch is increased to maintain the section between rivet holes when using large rivets, the difficulty of making tight repairs is still further increased.

Perfect tightness in a joint without theoretical correctness of proportion is of far more importance than correct proportions which may fail to secure perfect tightness. One boiler-maker may have appliances which will enable his men to make perfectly tight and sound work with rivets of unusually large diameter and pitch, and with which another maker would fail to make satisfactory work. The cases of boilers that have given way, and of expensive repairs that have been required through the rivets being too small, are very rare in comparison with the disasters that have occurred, and the expenses that have been incurred, through wasting of plates in consequence of leaky joints. No doubt it is advisable to keep up the ultimate breaking strength of the joint by increasing the diameter and pitch of rivet, but it is absurd to do it to such a degree as to risk making the plate weaker in the solid than in the joint, which it will inevitably become in time should the joint leak. If the wasting of steel plates occasioned by leakage took place only at the same rate as that of iron plates, the reduction in thickness with the former would render them less durable. But there is reason to believe that the wasting will be more rapid with steel in certain situations, hence the importance we attach to having perfectly

tight joints, lest the material should be blamed, instead of the design and workmanship, in the case of a boiler wearing out rapidly. The crippling strength of a ductile steel plate in front of the rivet may be considerably increased by increasing the lap or distance between the edge of the plate and center of rivets. With lap joints the practical objection to this is that beyond a certain limit, usually taken at one and a half times the diameter of rivet, the difficulty of making a joint tight by caulking or "fullering" increases with the amount of lap.

But in butt joints this objection is got over by increasing the lap of the plates only whilst retaining the usual amount of lap between the rivets and the caulking edges of the strips or welts. In double-riveted lap joints a considerable advantage in strength will be gained by increasing the distance between the lines of rivets in steel plates beyond the usual practice for iron plates, especially in zig-zag riveting.

STRUCTURES IN AN EARTHQUAKE COUNTRY.

By JOHN PERRY and W. E. AYRTON, Professors in the Imperial College of Engineering, Tokio, Japan.

From "The Architect."

When working at our paper on "A Neglected Principle that may be Employed in Earthquake Measurements," read before the Asiatic Society of Japan, May 23, 1877, we were led to consider how the effect produced by an earthquake on a structure is influenced by the time of vibration of the structure.

It follows from that principle that if a number of quickly vibrating bodies form part of the same structure, they all vibrate in much the same way; that is, the periods of their swings are all approximately equal to one another and equal to the periods of the earthquake; and although they differ in the amount of their motions these amounts and their differences are all exceedingly small; whereas if one or more of the parts of the structure are only capable of vibrating slowly, the periods of vibration of the different parts vary very much, the amounts of the motions are all comparatively great, and their differences are all relatively considerable. If, however, there is a sufficiently great viscous resistance to motion of such slowly vibrating parts, these parts will be found during an earthquake to behave much as if their natural periods of vibration were quick. Supposing the foundation of a structure to vibrate with the earth which encloses it, we see that a slowly vibrating structure which is fastened to these foundations is during an earthquake subjected to stresses which may be excessively great and of a very complicated kind, whereas a quickly vibrating structure is subjected to stresses which may be said to be determinate, and which are comparatively small. It is not here necessary to consider whether, as all the motions of a quickly vibrating body must be small, such a structure will be more comfortable to live in, because it is doubtful whether the annoyance produced by rapidity of shock would not more than counterbalance the annoyance of great but smooth motions. It is only safety we are here considering, and in this respect there can be no doubt of the superiority of rigid structures, or of structures having a sufficiently great viscous resistance to motion. We have made some calculations of the times of vibration of ordinary structures, such as well-built houses of stone and brick, chimneys, lighthouses, &c., and from these we see that the periods are all much less than what we judge from our experience is the ordinary period of vibration of earthquakes in Japan. Even two-storied houses built of wood if framed in the best way have quick times of vibration; such structures are, therefore, it seems to us, well capable of resisting the ordinary Japanese earthquake shock. As, however, we have not yet experienced the effects of a destructive earthquake, and as we presume that one of

the most important ways in which it may differ from ordinary earthquakes is in the suddenness of motion, or change of motion, it cannot be said that any ordinary structure has a quicker period of vibration than a destructive earthquake; consequently, if it be granted that stability depends on the structure having a quicker period of vibration than that of the earthquake, the stability of a building will be only relative; we can, of course, be sure that by making the walls of a building thicker and its height less that we add to its safety, but however far we may go in this direction we cannot be certain but that after all the earthquake period may be less than that of our building.

We must, therefore, content ourselves with saying that a slowly vibrating structure will probably get broken in its connections with the foundations if these be rigidly fixed to the ground, consequently (and we here oppose the practice of many architects and engineers) putting a heavy top to a lighthouse, the chimney of a factory, or other high building, must certainly take from its stability. And although the times of vibrations of ordinary brick and stone houses are very short, still in view of the possible great suddenness of a destructive earthquake we should advise that all buildings be kept as low and made as rigid as possible.

The argument used by engineers to support the practice above referred to of placing a heavy top on a chimney assumes that the shock is an impact, and, consequently, that a definite quantity of momentum is given to the structure, but it must be quite evident that it is the relative velocity of the base of the structure with regard to the other parts which is the fixed quantity, and, therefore, that the more massive the structure the more momentum enters it through the base.

There is no easy way of judging what are the forces which cause an ordinary Japanese house to return to the perpendicular position after it has received a push or blow, and so we cannot calculate its natural time of vibration; but it is well known that it vibrates very slowly, an ordinary Japanese two-storied house with the usual heavy roof taking perhaps four seconds to make a complete vibration. The restoring forces are due merely to stiffness of the joints, there being no rigid connection with the ground since the vertical posts of the house are all supported on detached stones, and there are also no diagonal stays in the building. Such a structure is therefore capable of being displaced very far from its position of equilibrium without fracture occurring, and as its time of vibration is very long, it has a very great amplitude of swing during most ordinary earthquakes; that this amplitude is not even greater is most probably due to the fact that there is a sort of viscous resistance to motion at all its joints. Such a viscous resistance must greatly diminish the motion, and will be especially useful in an earthquake consisting of regular vibrations, but the most severe test of such a structure consists in an earthquake shock which begins with a sharp impulse, or which has a very irregular motion. The slowly vibrating structure would register the shock in a longer period of time than that in which the blow was delivered, but it would probably have an exceedingly great first swing from its position of rest.

We think that the important elements of safety in ordinary Japanese structures is this viscous resistance which they oppose to motion, and which is mainly due to the great multiplicity of joints (all of which are compelled to move) and to the absence of diagonal pieces; for we deduced from the principle in our original paper, that if the restoring forces are weak there ought to be a great viscous resistance to motion if we wish the strains of the structure to be small. But it must be remembered that this safety is only gained by a very great expenditure of timber, so that although such slowly vibrating structures as many of the temples may be regarded as exceedingly safe during earthquakes, it must not be concluded that all heavily-roofed houses are secure.

The amount of momentum which has to be transmitted through the foundations of a building to the superstructure depends on the nature of the earthquake —that is, its suddenness and the amount of earth motion, as well as on the mass of the building, while the velocity of the foundations, if these are rigidly con-

nected with the earth, is independent of the mass of the building, an important fact to which we have already drawn attention. The earthquake energy gets destroyed by the interior portions of the earth as well as the mountains and buildings at its surface, not having exceedingly small periodic times of vibration, in consequence of which interference takes place at every surface of contact of the different portions. Of course, however, any one particular building will destroy only a very small portion of the whole energy of the earthquake vibration, so that its mass cannot in any preceptible way affect the motions of its foundations.

In the same way as we have shown that the more quickly a house is capable of vibrating the less is its motion relative to the foundation, we might arrive at the result that the smaller the natural period of vibration of the several portions of a body subjected to shocks the less internal friction must there be; and this conclusion is consistent with the well-known fact that there is more internal friction in non-homogeneous bodies, or rather, we should say, in bodies which, being non-homogeneous, have some of their materials only capable of very slow natural vibrations compared with the remainder.

We have no doubt but that with any given material whatever there is a best method of constructing buildings in an earthquake country. Thus with small stones set in bad mortar, or in no mortar, as in the buildings destroyed by the Neapolitan earthquake of 1857, the momentum which must pass through any level joint depends (1) on the short time t during which the foundations are acquiring a great velocity v; (2) on the mass of the building M above the joint; and (3) on the natural time of vibration of the portion of the structure between the given joint and the foundations. If this time of vibration is very short then the momentum Mv must be transmitted by the joint in the short time t—that is, the joint must transmit the great force $\frac{Mv}{t}$; whereas if the time of vibration of the building below the joint is considerable, the time of transmission of momentum is increased in a calculable way, say to the time nt, and hence the force transmitted by the joint becomes reduced to $\frac{Mv}{nt}$. It is for this reason that if we wish to drive in a nail without hurting the head with the hammer a block of wood is used as a cushion, the wood being of service because having an appreciable time of vibration it causes the duration of the impact to be lengthened, and so diminishes the force acting at any moment. In the same way the lower parts of a structure having appreciable times of vibration cause the earthquake shock to be altered in character, to be lengthened in time, and, therefore, diminished in intensity before it reaches the upper parts. Hence it is obvious that if small stones or bricks set in bad common mortar are our building materials it would be better to choose, for the site, a quaking bog, which was capable of supporting the weight of the building, rather than to build the house direct from a rocky foundation, or if the ground is firm there ought to be placed underneath the house a foundation of yielding timber, or some other method should be sought for by means of which the time of transmissions of momentum through the joints may be increased.

Thus there is a best time of vibration of the part of a structure below a joint, which depends on the strength of the joint; and if the basement has a time of vibration different from this, then, we should advise that the building be kept low. For example, it is desirable that houses with ordinary wall thicknesses built of bricks set in common mortar should not be more than one, or at the very most two stories high if there is a piled or concrete foundation; but if good cement be employed instead of bad mortar, then a height of two or three stories may be employed probably with comparative safety.

Again, the horizontal vibration of the ground is given up to a stone or brick building mainly by shearing stress communicated from course to course, a kind of stress which mortar is very unsuitable to transmit. Hence, a stone or brick building subjected to horizontal shocks ought certainly to be built with cement, and not with ordinary mortar. In fact, in every part it ought to be capable of resisting pulling as well as crushing stresses.

Every joint is a weak place, and it is evident that if, by increasing the size of the building, we diminish the area of joints we shall be increasing the stability. Now, in large masonry structures larger stones are as a rule employed, and the joints are made of less area. In this respect, then, may we say that large masonry structures built with common mortar are usually more stable than smaller ones.

It is quite evident that, as concrete can be obtained which will resist as great a tensile stress as ordinary brick itself, we shall derive great benefit from making all horizontal sections of a structure, which is composed of bricks set in good cement, as great as possible—that is, we shall find that the most suitable structure, if of brick or stone, for an earthquake country, should be composed of large stones set in good cement, with walls as thick as possible near the base, the thickness of wall at every place being roughly proportional to the mass of the building above that place.

As, however, the resistance to tension of timber is very much superior to that of cement or bricks, and as the mass of a timber building is small, a timber building with sufficiently strong joints must be very much superior to any structure of brick or masonry. And, for the same reason, a building of wrought iron might be made stronger still, and one of steel strongest of all.

Ordinary timber houses ought not to be too rigidly fastened to the earth; if the joints of the structure are made, however, very strong, and especially if wrought iron is used as well as wood, and if there is diagonal bracing, then the connections with the ground may be made more rigid. The stiffnesses of structures vary so much that we cannot give more definite rules than those contained in this short article, but it is obvious that our principle of relative vibrations may be easily applied to find the best arrangement in a structure for any given material, and with any given foundation.

STEEL SHIPS.

From "The Nautical Magazine."

We have reluctantly felt compelled to place the heading "Steel Ships" before this paper, but would desire to repeat our former observation that the new metal is not *steel* at all, but merely *ingot iron*. Our readers will pardon this reiteration when they are told that some great authorities on the subject have been so far led away by the name as to adduce experience of the wear of some decided steel ships which have been afloat for years, as proof of the reliableness of the new metal of an essentially different character, although bearing the same name. So far as its composition goes the new metal is rather an exceptionally pure iron than a steel, and for aught we know at present, may ultimately develope qualities the reverse of those of ordinary steel. The cautions recommended in using it, and the careful testing of each plate, are rendered necessary by the fact that in the present state of the new processes of manufacture we cannot without test be absolutely certain that the metal obtained is the real *bona fide* ingot iron or mild steel. Mr. Wimshurst suggests that in consequence of the great ductility of the new metal, the ordinary system of riveting may be found insufficient, but wisely does not lay down any rules to be followed, and concludes with the excellent practical suggestion that in all cases of passenger ships built of mild steel "frequent easily made surveys should be held during the first year," which surveys "need not be of such a character as to interfere in the least with the engagements of the vessel, but they will afford the Board a prompt and effective means of checking any evil which may be found to arise."

We have, in our present paper, to notice a lengthy and important commun-

ication made to the Institution of Naval Architects, by the Chief Surveyor to Lloyd's Registry, on the subject, and giving in great detail the result of a series of experiments instituted by the Committee of Lloyd's Register. Mr. Martell begins his paper by some remarks upon the prospects of the general adoption of the new material, and appears to regard the question as practically settled. He says, "The time has now come when it is said by many others, besides the manufacturers, that steel can be used with as much confidence as iron, and it is held that whilst the properties of mild steel are in every respect superior to iron, the cost, having regard to the reduced weight required, will warrant the shipowner, from a commercial point of view, in adopting the lighter and stronger material." We have also the important fact that during the last twelve months the Committee of Lloyd's have had before them proposals for 5,000 tons of sailing ships, and 18,000 tons of steamers, to be built of mild steel.

We are glad to hear that, so far as they have gone, Lloyd's fully agree with the Admiralty as to the practical value of mild steel. As regards its working qualities Mr. Martell produced a specimen "shingled" from cuttings of plates which were in use, and which had stood a tensile strain of 26 tons per square inch. Experiment proved that its behaviour in the fire and under the hammer was just that of ordinary iron: in fact, the welds were cleaner and more perfect. The first series of experiments were made upon the strength of riveted joints, the results being, briefly, that iron plates double-chain riveted with iron rivets, the holes being punched, developed a mean tensile strength of 17.9 tons per square inch. Steel plates connected with iron rivets gave out by shearing of the rivets at a strain 16.7 tons per square inch of rivet area, the strain upon the plate only reaching 15.3 per square inch. Steel plates connected with steel rivets developed a mean strength of 22.5 tons per square inch. The result of these experiments, if borne out by similar results with more extended experience, will be to prove, that in using iron rivets with steel plates we must have a larger proportion of rivet area to the plate area between the holes for double riveting, or the plates must be treble riveted unless it be found that steel rivets can be used with good results, in which case the ordinary scale of riveting will be sufficient. As regards the practical use of steel rivets, in addition to the practical experience at Glasgow to which we adverted in our former article, Mr. Martell states that they have been recently satisfactorily used in two steel vessels, built by Messrs. Laird, of Birkenhead. Special care must however be taken to make sure that the rivets are really *mild* steel, and even then it is desirable that they be uniformly heated, and not at too high a temperature. As an illustration of this, a case is adduced where some builders tried steel rivets, and found that after some landing edges of outside plating had been riveted, many rivets were broken mostly between the plates; and in this case iron rivets were ultimately used throughout the vessel. Subsequent experience has shown that mild steel rivets can be safely used by ordinary riveters, and what is more, with the ordinary rivet boys; and we must conclude, therefore, that the rivets which failed were not made of true mild steel.

A second series of experiments were undertaken with a view to ascertaining the relative effect of punching upon mild steel and upon iron plates. The results are thus summarized:

"1. That steel plates very thin suffer less from punching than iron.

"2. That the difference in loss of strength by punching on steel and iron does not appear sufficiently great to require special precautions to be taken for steel more than for iron in plates up to $\frac{8}{16}$ inch in thickness.

"3. That in plates above eight-sixteenths in thickness, the loss of strength of iron plates by punching ranged from twenty to twenty-three per cent., while in steel plates of the same thicknes it ranged from twenty two to thirty-three per cent. of the original strength of the plate between the rivet holes. An occasional plate, both of iron and steel, showed a smaller loss than the minimum stated, but they were exceptional cases.

"4. That by annealing after punching, the whole of the lost strength was restored, and in some instances greater

relative strength was obtained than existed in the original plates.

"5. That the steel was injured only a small distance around the punched holes, and that by riming with a larger drill than the punch, from $\frac{1}{16}$ inch to $\frac{1}{8}$ inch around the holes, the injured part was removed, and no loss of strength was then observable, any more than if the hole had been drilled.

"6. That in drilled plates, no appreciable loss of tensile strength was observed."

Mr. Martell then, at some length, considers the respective disadvantages of riming the holes or annealing the plates. He also shows that, even after allowing the twenty per cent. less scantling for steel, and supposing a further loss of thirty per cent. by punching the plates, as compared with the twenty per cent. loss due to punching in ordinary iron, the advantage is still with the steel. A better solution of the difficulty than annealing will probably be found in the use of some kind of punch which will distress the iron less than the common one does. Some of the experiments proved that the loss due to punching, when the patent spiral punch was used, was $2\frac{1}{2}$ tons per square inch less than with the common punch.

The second part of Mr. Martell's paper is devoted to the question of the relative cost of vessels built of mild steel and of iron, taking into the question the reduced weight of hull and consequent larger carrying capacity of the former. In the first place, he disposes of the objection that mild steel is of so much greater specific gravity than iron as to detract considerably from the advantage of the smaller scantlings offered by Lloyd's. It has been said that the difference was as much as 4 per cent., data furnished by Messrs. John Brown & Co., the well-known Sheffield firm, fix it at 2.66 per cent., and Mr. Bessemer states it to be still less. Mr. Martell goes into details as to the first cost, and subsequent yearly profit of a steamer 2,300 tons gross, supposed to be built for the Indian trade, and makes out that with a cargo of coals out and measurement goods home, the additional freight of the steel ship would just pay the percentage on her additional cost, but with a dead weight cargo out and home there would be a profit on the voyage of $6\frac{3}{4}$ per cent. in the steel ship as against $5\frac{1}{4}$ on the iron ship. With sailing ships the gain is not so clear, although, from the fact that a sailing vessel of 1,700 tons is now being built of the new material, it would appear that at least one large shipowner believes that even in the case of sailing vessels the additional freight would pay interest on the additional cost. Obviously a saving of weight in the structure is of very much more importance in a steamer than in a sailing ship; in the former, the machinery and coals absorb so much of the carrying capacity that the addition of a few tons to the freight gives a larger percentage on the total freight.

As regards the durability of the new material, Mr. Martell can tell us little more than has been known for some time past. We agree with him that the fact that the Admiralty are going to build some small torpedo vessels of brass or bronze instead of steel is nothing to the point. It has been found that some of the thin steel torpedo vessels have in a very short time become very much pitted; it must be remembered, however, that they are only $\frac{1}{16}$ inch thick, and an amount of deterioration hardly noticeable in another vessel would be serious in them. Less to the point are the other remarks as to the durability of some vessels built of steel some years ago, and which have worn well. It cannot be too much insisted upon that these vessels were built of *bona fide* steel, whereas the new metal, mild steel, in some of its properties, is much more analogous to wrought iron than to steel. Especially is such the case in the most important feature, as regards decay. The chemical analysis of mild steel shows a larger percentage of pure metallic iron than is found in any commercial wrought iron.

Probably with the increased demand for mild ship steel the cost of production may, in a few years, be so diminished that it may successfully compete with wrought iron for all kinds of ships. At present it will probably be used in many steamers, more especially in vessels designed for speed, in which, as compared with ordinary steamers, every ton of increased freight is of as much greater importance, as in the comparison between ordinary steamers and sailing ships.

THE BRAKE AS A DYNAMOMETER.

From "The Engineer."

THE friction brake is so generally regarded as an essentially accurate instrument for ascertaining the power developed by a steam engine or water wheel, that it requires some courage even to suggest that it is perhaps not quite such an instrument of precision, after all, as some persons would have us think. The friction brake is more used by builders of portable engines than by anyone else.

There is scarcely a respectable agricultural engineering works in the kingdom in which the friction brake is not regularly and frequently employed. But the great majority of mechanical engineers engaged in the construction of marine engines, locomotives, or stationary engines of large power, know nothing practically about it. It is, therefore, to the experience of agricultural engineers that we must turn for such information as may enable us to form an estimate of the true value of the friction brake as a power-testing machine; the remainder of the engineering community can, as we have said, tell us nothing whatever that is not theoretical about it. Now it so happens that many agricultural engineers say that they have found by experience that the friction brake is by no means so precise an instrument as theory would have us believe. Indeed, unless these gentlemen are wholly mistaken, the brake may, theory to the contrary notwithstanding, prove very deceptive. Everything, it it is said, depends on the condition of the brake. If that is perfect, then a high duty can be got from an engine; if it is imperfect, then the performance of the engine will be bad. To explain our meaning, it is necessary to go back to the days when prizes were given by the Royal Agricultural Society for portable engines. The competing engines were made and tested daily for months before they came to the public trial. Now, it was well known to those who superintended the daily runs made with a racing portable engine, that whereas on some occasions a run of, say, four hours could be obtained with 14 lbs. of coal per brake horsepower, on other days the run would not exceed three and a-half or three and three-quarter hours, and there was no possible explanation of the circumstance save that the brake did not work smoothly. Carrying this experience into practice, engineers always did their best when competing publicly, to get a brake which had been worked until it was in perfect order; and some of the most eminent authorities on racing portable engines maintained that the difference between a brake in what is known as a good condition and one in bad condition may be such as to affect the length of a run by from five to ten minutes.

Such conclusions and experiences as we have just noticed are totally opposed to the received theory of the friction brake; yet it is impossible to ignore them, and it may be found that the apparent incompatibility may be reconciled by adding something to the theory which is in no way opposed to physical truth. The friction brake or dynamometer consists of a smooth pulley some 5 feet in diameter, round which run two hoops of iron lined with blocks of elm, beech, or willow. The hoops can be tightened by a hand screw, and when so tightened would, if permitted, revolve with the pulley. To prevent this they are fitted with a simple lever arragement by which the straps are slackened if they move through a short distance with the pulley, and at one side of the ring of wood blocks is suspended a weight, calculated according to the power required. This weight is kept in suspension the whole time that the pulley is running, its weight being just sufficient to equal the frictional resistance of the blocks on the rim of the pulley. This being so, it is assumed that the resistance offered to revolution by the apparatus will exactly equal the power that would be required to wind the weight on the brake out of a pit, say, of great depth. Let the distance from the point at which the break load is suspended to the center of the brake pulley shaft be such that, using it as a radius, a circle 33 feet in circumference would be described, then for every 1 lb. of brake load and one revolution of

the brake pulley 33 foot-pounds of work will be done. Let the revolutions of the brake be 100 per minute, then every pound of brake load represents $33 \times 100 \times 1 = 3300$, and every 10 lbs. of brake load becomes $33 \times 100 \times 10 = 33{,}000$ foot-pounds per minute=one horse power. It will be seen that the apparently absolute measure of the work done is the load on the brake and the surface speed. The maximum resistance the engine can have to overcome is measured by the weight, because if the hand screw is tightened the weight will rise, and would be carried round with the wheel but for the levers before referred to; while, on the other hand, if the straps were released, the weight would fall a little until the straps automatically tightened it again. According to theory, again, the condition of the brake has nothing to do with the matter. If the surfaces of the pulley and the wood blocks are rough, then the hoops must be left a little slack. If, on the other hand, the surfaces are beautifully smooth and well oiled, then the hoop must be tighter, but in either case the resistance offered to the engine is precisely the same, and is measured by the weight which hangs balanced in mid-air while the engine is running. There can be no doubt that this reasoning is extremely plausible, and would be quite convincing if it only covered the whole of the ground to be traversed. But let us ask ourselves what becomes of the power developed by the engine? No useful work is done; the weight is not lifted, and the only reply is that the power is transformed into heat; that is to say, the engine heats up the brake pulley and its connections, and it also heats up the water or oil used for lubrication. This heat is dissipated by conduction and radiation. It amounts to 42.75 thermal units per horse-power per minute.

An engine working up to 20-horse power develops as much heat in the brake as would rise from 62° to the boiling point 342 lbs., or say 34 gallons of water per hour. All this is quite intelligible, and a little examination will show that the engine, instead of lifting a weight, works against friction, and it is assumed that the weight is a precise measure of the amount of friction, or, to speak more accurately, of the quantity of heat which will be transferred per hour from the engine to the brake, and thence to the air and the lubricants. On this point the whole theory of the friction brake really turns, and unless it can be proved that a given weight resting on a polished surface running at a given speed beneath it can produce an amount of heating which is invariable under all circumstances for the same conditions, then the theory of the brake must be regarded as incomplete. Hitherto almost all writers on this subject entirely neglect the consideration of the heat imparted to the brake. They allude to it, indeed, but only incidentally, and they say nothing whatever concerning the relation between the brake load and the heat developed. They content themselves with considering the duty done by the engine to be precisely similar to the work of lifting a weight, whereas they are totally dissimilar, and if it could be shown that under certain conditions a given brake load would convert greater or lesser quantities of engine power into heat, then the idea that the friction brake is thoroughly reliable dynamometer would have to be abandoned. It is well known to all who have had experience that friction brakes will run sometimes hot and sometimes cool, and, according to those whose experience constitutes the best authorities, *that the cooler a brake runs the smaller is the power required to work it.* If this be true, then it is evident that the usually received ideas concerning the merits of the brake as a dynamometer must undergo some modification.

It will be understood that we have advanced nothing concerning the friction brake which will not be confirmed by many engineers who have used it much. It is difficult to reject as valueless opinions which we have heard expressed over and over again for years, and the accuracy of which is suggested by our own experience. All that we have now endeavored to do is to show how it may be possible to reconcile theory and practice. It is certainly possible to conceive that under all possible circumstances the coefficient of friction need not bear an invariable relation to each other. Let us suppose that the coefficient of friction of well lubricated wood blocks is $\frac{1}{50}$, and that the weight to be supported is 100 lbs.

then the blocks must be applied to the wheel with a force of 5000 lbs., and the heat developed on the brake per minute will be 427.5 units. Now it is absolutely certain that the conditions of speed, load &c., being constant, the rate of conversion of power into heat must also be constant. In other words, is there an invariable relation between frictional resistance and heat developed? That an approximate relation does exist we do not for a moment question, but that anything like an invariable correspondence can be proved to exist, is open to question. Those who have the means of settling the point by actual experiment should do so. The friction dynamometer is no doubt a substantially accurate machine; but if a legal difficulty arose to-morrow about the power of an engine, a jury would soon have reason to believe that even under the best arrangements the friction brake may be as much as perhaps 10 per cent. wrong in its indications.

REPORTS OF ENGINEERING SOCIETIES.

THE INSTITUTION OF MECHANICAL ENGINEERS, held meetings in Paris in June. The following papers were read:

Further Researches on the "Flow of Solids"; by M. Henri Tresca, President of the Société des Ingenieurs Civils.

On the Hydraulic Machinery at Toulon Dockyard; by M. Marc Berrier Fontaine, Ingénieur de la Marine, Toulon.

On Mechanical Traction upon Tramways; by M. Anatole Mallet, of Paris.

On the Greindl and other Rotary Pumps; by M. L. Poillon, of Paris.

On the Vapart Disintegrator; by M. Prosper Closson, of Paris.

On Compound Engines fitted with Correy's Variable Expansion Gear; by Mr. Thomas Powell, of Rouen.

On the Effect of Brakes upon Railway Trains; by Captain Douglas Galton, C.B., F.R.S., of London.

On Lighting by means of Electricity; by M. Hippolyte Fontaine, of Paris.

IRON AND STEEL NOTES.

ANALYSES OF RUSSIAN IRON.—Mr. Sergius Kern has written from St. Petersburg commenting upon the remarks of Mr. E. Riley, that he was astonished that most of the steels, the analyses of which appeared in Mr. Kern's late paper, contained only traces of Ph. and S Mr. Riley also complained that the percentage of Mn. was too low in the analyses, and added that perhaps Mr. Kern used inferior methods for the detection of Ph. S. and Mn. The following are the answers of Mr. Kern: "(1) The steels in question were prepared from Oural pig-irons; most of them, indeed, contain only *traces*, or *nil*, of Ph. and S. Charcoal is used as fuel. (2) The methods I use belong to Eggertz, and may be found in his classical manual 'Om Kemisk profning af Jern, Jernmalmer och Braenn materialier.' Using the methods of the well-known Professor V. Eggertz, I cannot understand why I should prefer other methods. (3) As for the low percentage of Mn., I will only mention that I cannot understand what Mr. Riley wishes, as it is not my fault that the Russian steels contain such a low percentage of Mn."

AT the Philadelphia Exhibition, it will be remembered, an International Committee, consisting of commissioners who were over reporting for the different countries, had a discussion on the classification of iron and steel, and proposed new definitions. Among those on the committee were Mr. I. Lowthian Bell, M.P., F.R.S., and Dr. Reuleaux, of Berlin. The German Ironmasters' Association has, according to the *Iron and Coal Trades Journal*, just had this classification under discussion, and resolved:—(1) that a general classification of iron and steel is neither necessary nor useful; (2) that the tests now customary for testing iron and steel goods—hammering, bending, and loading for rails, bending for axles, pulling for sheets, &c.—are sufficient; (3) a specification of limits of value of the properties of iron and steel goods in reference to their uses is desirable; (4) that a further prosecution of the experiments hitherto conducted by the association, with common commercial irons, is therefore desirable, in view to an eventual special classification of railway material; (5) that State testing be placed under the control of a commission, consisting on the one part of delegates chosen by consumers and producers alike, and, on the other, of approved men of science; (6) quantities of metal in railway contracts to be determined by ironmasters conjointly with the railway engineer; (7) that the proposal made by Dr. Reuleaux, to draw up a table of properties, and stamp goods with a mark corresponding to a designation in the table, is impracticable.

SIEMENS-MARTIN METAL RULED TO BE STEEL. —Secretary Sherman has sent a letter to the Collector of Customs at Boston, Massachusetts, in which the vexed point of how Siemens-Martin metal is to be taxed, is disposed of. The text is as follows:—"The Department, by decision of December 1st, 1874 (Synopsis 2025), held that metal produced by what is known as the 'Martin-Siemens process' should be charged with the duty imposed upon steel, such process being considered a steel-making process, designed only to produce an article having the 'quality of steel.' Subsequently, upon further consideration, and upon additional facts at that time submitted, the Department, by letter of July 14th, 1876 (Synopsis 2891), expressed its conviction that both iron and steel are produced by the Martin-Siemens process, and that, consequently, the fact of manufacture by that process was not of itself

conclusive ground for classifying the product as steel; but that the question whether any particular importation was iron or steel was one of fact to be determined by the appraisers. It has recently been ascertained that a want of uniformity has prevailed at the ports of New York and Boston in the classification, since the later decision, of importations of metal produced by the Martin-Siemens process; metal of that character, and similar in every respect, having been, without exception, classified at the first-named port as steel and at the latter as iron. In view of these facts, the Department has again had the matter under consideration, and has submitted the question of the character of this metal to experts, metallurgists, and the most prominent manufacturers of, and dealers in, iron and steel in the United States. A careful consideration of the reports and opinions of these persons satisfies the Department that the Martin-Siemens process was intended to be, and is essentially, a steel-making process, and that the product of such process must consequently be steel or an article possessing the general characteristics of steel, and used for the purposes to which steel is applied In confirmation of the correctness of this view, it may be stated that the classification at the port of New York of the metal in question as steel has been accepted without dissent by importers of that city, and that protest against payment of duty exacted on such classification has in no case been made. After a full examination and consideration of all the facts and information bearing upon the question at issue, the Department is of opinion that the classification as iron, of metal produced by the Martin-Siemens process, is erroneous, and that all metal produced by that process should be hereafter classified as steel, and assessed with duty accordingly. The decision of the Department of the 14th July, 1876, hereinbefore referred to, is therefore revoked, and decision 2025 will be regarded as in full force."

RAILWAY NOTES.

The St. Gothard Railway Co. finds some difficulty in obtaining the money necessary to complete its work. According to the original understanding under which the undertaking was begun, Italy was to have contributed $9,000,000; Switzerland, $4,000,000; the North German Confederation, $2,000,000; the Grand Duchy of Baden, $600,000, and the other German States the additional cost. Now Switzerland is asked to contribute as a nation, instead of by States, $1,300,000, on condition that the Northern & Central Railway Co. gives $300,000 more, which, it is estimated, will complete the road. Whether these subsidies are in addition to those originally agreed upon does not appear in the dispatch. The road will connect Luzerne and Milan by rail, and the division of cost between the nations is supposed to represent the proportion of benefits to be derived by each from its construction. It now requires fifteen fifteen hours to cross the Alps by the St. Gothard pass in the diligence from Fluelan to Bellinzona.

In discussing the recent half-yearly report of the Great Indian Peninsular Railway, Colonel Jas. Holland said:—"In the corresponding half of last year the proportion of English to native fuel used was 84 per cent. of English to 16 per cent. of native coal. Last half-year the proportion was 68 per cent. of English to 32 per cent. of native, so that we are coming to use native coal more considerably. I only wish I could say that the native fuel was as good as the English. It is, however, excepting that from Bengal, very inferior, but that, though good, is as dear as coal from England. It would shock any one accustomed to English coal to see with what rubbish from Wararo we work our line. It produces a vast quantity of sparks, and a considerable portion of the compensation paid for damage to goods has been owing to burning inferior coal. We find with the new and powerful engines now day by day coming upon the line that they puff and blow less; the sparks are consequently fewer. We may now be said to be using about one-third native coal; last year we used about 10,000 tons of native, this half-year we shall probably use about 30,000 tons."

The Belgian Grand Central Railway Company, in their annual report for 1877, publishes some statistical tables showing, for the period from 1865 to the end of 1876, the number of rails removed from the track, of those deteriorated but not removed from the track, removed and deteriorated, the number of remaining in the track uninjured, both of iron and steel. From these tables it appears that all the iron rails used before 1873 are of bad quality, except those laid in 1867, 1869, and 1870; these latter are hammered rails. Of the rails laid since 1873, the quantity removed is insignificant; this is because for the past few years the management of the Grand Central makes sure of the quality of the rails, and purchases only of works which offer sufficient guarantees under this head. The quantity of rails in the track on the last of January, 1878, was 37,000 tons of iron, and 3385 tons of steel rails, and to maintain this track since 1865 has required 55,000 tons of iron, and 3388 tons of steel rails. Thus already 18,000 tons of iron rails have been renewed, and only *three* tons of steel. The greater part of the iron rails renewed are of those delivered in the years 1865, 1866, 1868, and 1871, which have been the worst, for of the 18,000 tons of iron rails removed, 12,600 were of the rails laid during these years. There have been broken 97 rails in all—94 of iron and 3 of steel. Comparing these figures with the whole number of rails of each kind in the tracks, we find that 0.04 per cent. of the total number of iron rails have been broken, and 0.02 per cent. of the total number of steel rails—that is, the number broken is in the proportion of steel to two iron rails; and 68.04 per cent. of the breakages have been at the fish-bolt holes.

The Railroads of the United States in 1877.—From advance-sheets of *Poor's Manual* (the eleventh annual number) we take the following:

"The depression of the three previous years still continues. Not only has there been a considerable decline in the construction of railroads, but the earnings also show a larger relative decrease than at any period since the first publication of the *Manual*. The number of miles of railroad opened during the year 1877 was 2177, against 2657 for 1876, 1758 miles for 1875, and 2305 miles for 1874. The largest number of miles built has been in New York and Pennsylvania, and in narrow-gauge lines in Ohio, Iowa, and Texas. No new lines of any considerable magnitude have been undertaken. The tables which follow will show in what sections there has been any considerable increase.

"The gross earnings of all the roads whose operations have been reported have equaled $473,909,272, against $497,257,959 for 1876 and $503,665,505 for 1875. The general result of the operations of our railroads for the last seven years is shown in the following statement:

STATEMENT SHOWING MILES OF RAILROAD, CAPITAL ACCOUNT, EARNINGS, ETC., FOR SEVEN YEARS.

Year.	Miles operated.	Capital and funded debt.	Earnings.				Dividends.
			Gross.	Net.	From freight.	From passengers.	
		$	$	$	$	$	$
1877	74,112	4,508,597,248	473,909,272	170,976,697	342,859,232	130,050,050	58,556,312
1876	73,508	4,468,591,935	497,257,959	186,452,752	361,137,376	136,120,583	68,039,668
1875	71,759	4,415,631,630	503,665,505	185,506,438	363,960,234	13-,105,271	74,294,208
1874	69 273	4,221,763,594	520,466,016	189,570,958	379,466,935	140,999,081	67,042,942
1873	66,237	3,784,543,034	526,419,935	183,810,562	389,035,508	137,384,427	67,120,709
1872	57,323	3,159,423,057	463,241,055	165,754,373	340,931,785	132,309,270	64,418,157
1871	44,614	2,664,627,645	405,399,208	141,746,404	294,430,322	108,898,886	56,456,681

"It will be seen by the above that the gross earnings have fallen off $25,348,687, and the net earnings $15,476,055, as compared with 1875.

"The ratio of net to gross earnings was 36.16 per cent., as against 37.5 per cent. for 1876, equal to an increase of 1.36 per cent. in the operating expenses, as compared with the preceding year. The decrease in earnings from freight has amounted to $18,278,154; and in passenger traffic, $6,070,533; the percentages of decrease being respectively 9.5 and 9.7 per cent The dividends have fallen off $9,483,356; and are less than for any year since 1871. The total amount of capital stock on which dividends were actually paid was $835,038,896, giving an average rate of seven per cent. No dividends were paid on any of the railroads in the States of Arkansas, Colorado, Florida, Kansas, Louisiana, Mississippi, Missouri, Nebraska, Oregon, Texas and Vermont—nor excepting on leased lines in Iowa and Minnesota.

"The principal decrease in earnings has been in the Middle States, due partly to the depressed condition of the coal trade, and partly to the falling off in passenger earnings as compared with 1876, the Centennial year.

"The elaborate tables heretofore printed in the *Manual* are omitted this year; but the final results, the only important feature, are given in full detail. There is added a table reducing these results to the unit of 100. From this it will be seen that for each 100 miles of railroad in the United States there are 22.8 miles of second track, sidings, etc.; 20.1 locomotives; 15.2 passenger cars; 4.7 baggage, mail, and express cars; and 495.3 freight cars of all kinds.

"The capital stock aggregates, $2,921,507 for each 100 miles; the funded debt, $2,848,308; the floating debt, $300,078; and the total cost of construction and equipment, $6,069,893; equal about to $60,699 per mile of completed road.

"The gross earnings per mile were, $6380.94; operating expenses (63.85 per cent), $4074; net earnings, $2306 90. Interest paid on bonds per mile of road, $1248.04; dividends paid on stock, do. $739.52. The ratio of interest paid to total funded debt was 4.39 per cent; of dividends to aggregate capital stock, 2.53 per cent. In 1871, with only two thirds as many miles of railroad in operation, and a little more than one half the capital stock, the dividends aggregated $56,456,681, equaling 4.19 per cent. of the capital then invested.—*Engineering and Mining Journal.*

IT is but a few years since the idea of bridging the Mississippi and Missouri rivers was held to be both impracticable and outrageous, as contemplating an infringements on the rights of navigations, and terrible pictures were drawn of the damage which would ensue to the boating and rafting interests if a single structure could be thrown over one of those streams. But the locomotive could not be kept back; one bridge was built and then another, and now there are no less than eleven structures—ten upon piers and one a pontoon bridge—spanning the father of waters between Winona and St. Louis. From a lengthy re-

port from a United States board of engineers, the *Railway Age* quotes the following list of these structures and their sizes:

At	When built.	No. spans.	Longest span, feet.	Draw.
Winona	1871	16	240	160
La Crosse	1876	10	240	160
Prairie du Chien	1875	—	Pontoons.	—
Dubuque	1868	8	240	160
Clinton	1865	14	180	118
Rock Island	1871	7	250	160
Burlington	1868	10	200	160
Keokuk	1870	12	240	160
Quincy	1868	24	160	160
Hannibal	1871	8	240	160
Louisiana	1873	11	256	200

ENGINEERING STRUCTURES.

THE Emperor of Brazil has recently written an autograph letter to Mr. James B. Eads, soliciting his advice in connection with the contemplated improvement of some of the great rivers in that country.

THE SUTRO TUNNEL.—This remarkable engineering enterprise will soon reach a successful termination.

The famous Comstock lode has been worked at a great expense, partly from difficult drainage and, partly from the high temperature, (120° F.)

Surveys made some years since indicated that a tunnel, nearly four miles in length, would lessen the difficulties and permit working to greater depths than would otherwise be possible.

The State of Nevada, in 1865, granted to Adolph Sutro the exclusive right for fifty years to run the proposed tunnel. A contract was made with all the leading companies, in which they agreed to pay $2 per ton for all the ore extracted after the main tunnel is complete and actually drains the mines; or, if they are not drained, then after a lateral drift reaches any mine. In 1866 the Federal Government granted the right of way through the public domain for seven miles along the Comstock lode; also the right to select 1,280 acres of land at the mouth of the tunnel, and the right or title to the mines for 2,000 feet on each side of the tunnel. All the mines of the Comstock lode are made tributary to the tunnel, the same as in the contract mentioned above. These measures were carried in response to recommendations and memorials signed by all the prominent mining officials, bankers, etc., on the Pacific Coast.

The tunnel has been in progress some eight years, and not far from $3,000,000 out of about $4,000,000 required to complete the work and its railway connections have been expended up to this date.

FOUNDATIONS FOR BRIDGES.—The system of making foundations for bridges in marshy soils, adopted by French engineers, in the case of the Charentes Railway—a line which crosses a peat valley to the junction of two small rivers—seems to have solved the problem of what is required in such cases. The thickness of peat at this point was so great that any attempt to reach the solid ground would have been extremely expensive. In order, therefore, to obtain a good support for the bridge, two large masses of ballast, accurately rammed, were made on each bank of the river, and a third on the peninsula between the two. The slopes of these heaps were pitched with dry stones, for preventing the sand from being washed away by the rains of by the floods in the rivers. Over the ballast a timber platform was laid, this platform carrying the girders of the bridge, which has two spans about sixty feet each. When some sinking down takes place the girders are easily kept to the proper level by packing the ballast under the timber platform—this platform packing being made by the plate-layers with their ordinary materials.

In another case—that of a railway in Algiers—a different plan of engineering was resorted to. This road crosses a peaty plane nearly a mile broad, the floods and elasticity of the ground preventing the formation of any embankment. The road was to be carried over a viaduct across the valley, but the foundations of this viaduct presented serious difficulties, the thickness of peat or of compressible ground being nearly eighty feet. It was quite possible to reach the solid ground with cast-iron tubes sunk with compressed air, or any other system; but neither the implements, the workmen, nor the material for such an undertaking were accessible in that region.

Under these circumstances, the engineers began boring holes ten inches in diameter down to the solid ground; these holes, lined with thin plate iron pipes, were afterward filled with concrete up to the very level of the ground. Each of these concrete columns bears a cast iron column, these columns being braced together in a suitable manner, thus supporting the girders of the viaduct.—*Railway Review.*

WIRE TRAMWAY WORKED BY WATER WHEELS.—The tramway connecting the town of Lausanne with its harbor Ouchy, on the lake of Geneva, consists of two lines of rail, and two trains which are connected by a wire rope. At the top of the tramway the rope passes over a winding drum, through which the trains are put in motion. The two trains keep each other in equilibrium, the one ascending upon one line while the other descends on the other line, and *vice versa*.

The tramway is 1,650 yards long, and leads in a straight line from Ouchy up to Lausanne, passing on the way a tunnel several hundred yards in length. The steepest gradient is 1 in 9.

The winding drum is driven by two Girard turbines, which work under a head of 393 feet; they are made of brass on account of the high velocity of the water, due to the great head; they have a diameter of seven feet four inches, and run at a speed of 170 revolutions per minute. The water can easily be turned on and off the turbines by means of circular slides worked by hydraulic gear.

The two turbines are fixed upon a horizontal shaft, which carries also a brake wheel, the band of which is worked by gears similar to

the slides, and spur gear for transmitting the motion to the winding drum.

The winding drum is 19 feet 8 inches in diameter and 13 feet long, and is covered with wood lagging. As it has to transmit by mere friction a force 180 H.P., making at the same time only a few revolutions per minute, the following arrangement to produce the necessary friction has been contrived by M. Callon, the designer of the tramway: The winding drum is placed in a position parallel to the direction of the tramway and considerably lower than the level of the rails; the rope is wound on the drum in two coils, and above the drum; the two ends of the rope are made to pass over two guide pulleys, which stand at right angles to the drum, and are carried in sliding bearings. By means of bevel gear and screw spindles, these pulleys are made to move to and fro along the winding drum, thus forcing the rope to travel continually from one end of the drum to the other, and preventing the surface of the latter from being worn smooth, as it would be if the coil were always on the same spot.—*Review.*

PUBLIC WORKS IN FRANCE.—M. de Freycinet, Minister of Public Works, is an able and ambitious man, and has lost no time in framing a project which was well calculated to excite the imagination of the French people. At the close of 1877 he had developed his plans, and on the 2d of January a project was laid before the Marshal President, which proposed to expend one hundred and twenty millions sterling upon the development and reorganization of the railway system in France. Nor was this all. Some days later a supplementary project was presented, demanding the expenditure of an additional forty millions sterling upon canals. An expenditure of one hundred and sixty millions sterling would be an arduous enterprise for even the most wealthy and actively prosperous of countries, but in a country which has been so depleted of capital as France has been within the present decade, it is a proposal demanding peculiar courage and coolness in those who make it. As must have been expected, it was assailed, not only by M. Rouher and others in the interests of the monopoly which the existing great companies practically enjoy, but by some advocates of the smaller companies, who are anxious to make better terms for their clients. M. de Freycinet's answer is practically a plea in "confession and avoidance." He admits that if the whole sum of 160 millions sterling were to be withdrawn at once from active use, and sunk in the construction or working of unproductive railways, the danger of a financial crisis might become imminent, but he points out that the expenditure will be gradual—will be spread, indeed, over ten years or more. Six commissions—one for each of the *réseaux* worked by the great companies—have been appointed to inquire whether the main systems of each of those companies may not be extended, and in a few weeks it is anticipated that they will have prepared their reports. When they have reported, the Ministry will be able to state with fair precision what the extent of the national railway system will be. The conjectures of well informed persons are to the effect that the Ministry, after the above-mentioned reports have been received, will state that provision must be made on national grounds for the maintenance of some 38,000 kilometers of railway in France. Of these "national lines" only about 21,000 kilometers are at present in working order; 5000 kilometers have been sanctioned by the Chambers, and private enterprise has undertaken 2000 more. But supposing all these projects to be carried out, there would still remain a deficiency of from 8,000 to 10,000 kilometers, for which new and additional provision must be made. In the same way, M. de Freycinet contends that the extension of the canal system ought to be provided for, and the reports of five commissioners appointed to inquire into the artificial waterways of the five great "catchment basins" of France will ultimately guide the Chambers. An expenditure of 30 millions on new canals and on the completion of old work, and of ten millions on the deepening and improvement of ports—such is the outline of M. de Freycinet's scheme, of which the bill now before the Chamber of Deputies is only the first and most modest installment. As for the financial plans with which the Minister of Public Works hopes to meet the new burdens he would impose upon his country, they are important enough to require separate consideration. It is enough to say now that they would involve the addition, according to M. de Freycinet's calculations, of seven millions sterling a year to the taxation of France.—*The Standard.*

ORDNANCE AND NAVAL.

NEW GATTLING GUNS.—Mr. Ackers, agent of Dr. Gattling, inventor of the mitrailleuse, tried at Sealand Range, Chester, recently, in the presence of Captain Rogers and a number of officers and men connected with the pensioners now up for training, three new patent Gattling guns, which have never before been tried in England. The mitrailleuses were first tried at 1000 yards range, Mr. Ackers working the machine. When everything had been arranged, the signal was given, and the weapon literally poured out a hail of bullets, the majority of which struck the canvas target and tore it all to shreds, and penetrated quite through 2-inch oak supporting poles. Accurate time was kept by Captain Rogers, and it was ascertained that the mitrailleuse fired 1000 rounds a minute, which is 300 to 400 rounds a minute faster than any other Gattling gun. Experiments with the weapon were then tried at 800 and 600 yards range, and the way in which the bullets were hurled at the target, and the marvelous precision with which they struck it astonished every one present. The sergeant-major who was working it said that a sparrow must have been killed flying across the line of fire: the bullets which fell a little short tore up the clods of earth and hurled them right over the target into the workmen's retreat. It was the opinion of competent judges that this is the most destructive weapon ever invented.

THE LOADING OF HEAVY GUNS.—To facilitate the loading of heavy guns it has been of advantage to enlarge the bore at the muzzle by half an inch or more by scooping out half an inch or so of metal for a depth of about two inches. This process is to be termed "bell mouthing," and it is to be applied to all the guns in the Service of ten inches and upwards. Artificers are being sent in various directions to make the alterations in the guns at the several forts and stations.

A NEW EXPLOSIVE.—It was stated at the last meeting of the Royal Dublin Society that a new explosive agent has been discovered by Professor Emerson Reynolds in the Laboratory of Trinity College, Dublin. It is a mixture of 75 per cent. of chlorate of potassium with 25 per cent. of a body called sulphurea. It is a white powder, which is very easily prepared by the mixture of the materials in the above-named proportions. The new powder can be ignited at a rather lower temperature than ordinary gunpowder, while the effects it produces are even more remarkable thon those caused by the usual mixture. Dr. Reynolds states that his powder leaves only 45 per cent. of solid residue, whereas common gunpowder leaves about 57 per cent. It has been used with success in small cannon, but its discoverer considered that its chief use would be for blasting, for shells, for torpedoes and for similar purposes. Dr. Reynolds pointed out that one of the advantages this powder possesses is that it can be produced at a moment's notice by a comparatively rough mixture of the materials, which can be stored and carried without risk so long as they are separate. The sulphurea, the chief component of the new explosive, was discovered by Dr. Reynolds about ten years ago, and could be easily procured in large quantities from a product of gas manufacture which is at present wasted.

A NEW ITALIAN IRONCLAD.—The ironclad Dandolo, which was launched at La Spezia, on Wednesday, is a sister ship of the Duilio, now completing for seas for the Italian Government. Both of them are to be armed with 100-ton guns, and destined to carry armour no less than 22 inches in thickness; so that, in point of armament, these Italian men-of-war bid fair to be the most formidable afloat when they are finished. Our Inflexible will not be so heavily armed as either the Dandolo or Duilio for her turrets are fitted to contain each of them a pair of 80-ton guns, while the metal of the Italians consists of four 100-ton cannon. On the other hand, the iron walls of the British ship are a little stouter, being 24 inches instead of 22 The Italian armor was devised to keep out shot from any cannon of less power than that carried by the ship itself, and this the plating practically does. The Duilio armour is capable of repelling all shot with the exception of that from an 80-ton or a 100-ton gun. The penetration of a 38-ton gun, the heaviest in our service at this moment, is set down at 19½ inches at a short range, and with the employment of a battering charge, and the Duilio, has its turrets protected with 22 inch plates. On the other hand, the 80-ton gun would make as little difficulty in getting through 23 inches of iron as 24, and there is little doubt nothing less than three feet of iron can be depended upon to stop the terrible blow of ¾ ton of metal hurled through the air at a speed of nearly a mile per second. The Italians have not been daunted, however. They have already set to work, and are now constructing two ships to carry armour plating capable of resisting any gun in existence. They hope to build a pair of turret vessels armored with 2 feet of solid iron, and to carry cannon of perhaps 200 tons. The names of these stupendous floating structures are the Italia and the Lepanto, but in the meantime Italy possesses in the Dandolo and Duilio two men-of war destined to carry heavier metal than any ship in the British Navy. The Dandolo was planned by the Commendator Brin, the ex-Minister of Marine The plates were constructed by Schneider of Creusot, and the engines by Maudslay, of London.

THE NEW FIELD GUN.—The new field-gun, which had, by a course of experiments extending over more than two years, undergone an evolution from a 9-pounder to a 12-pounder without enlarging its bore or materially increasing its weight, has undergone a further and final development, and may shortly be expected to appear as the model field-piece of the British Artillery in the shape and weight of a 13 pounder. Experience has proved that much of the value of a good field-gun lies in the length of barrel, and accordingly the 13-pounder, although no thicker than a 9-pounder, will be considerably longer than even the 16-pounder, the heavy gun of the field batteries of artillery, the efficiency of which is now admitted to have been sacrificed to the prejudice which existed at its introduction against impairing its symmetry by elongating the muzzle. The 13 pounder has undergone a rigid course of experiments. It is a compound of all the recent inventions, and it has produced splendid results.

SHELL PENETRATION.—Some trials of shell penetration of a very important character have lately been conducted at Shoeburyness under the direction of a committee appointed for the purpose. The experiments were in the nature of a competition between the shells of different makers, and hence, as they are to be resumed, it is not thought desirable that precise details should be published concerning them until they are completed. The general results obtained up to this point may be briefly stated. The object was to ascertain what shell would combine with the greatest power of penetration the power to retain its bursting charge in a state of efficiency. For this purpose the most eminent firms in England and on the Continent were invited to supply six shells each for a 9-inch Woolwich gun, the only restriction being that they were all to be of the same exterior and interior dimensions, the material and mode of manufacture being left to the discretion of the makers. Five English and four foreign firms entered into the competition, and three varieties of projectiles were sent from Woolwich—an ordinary Palliser chill-

ed iron shell, an improved chilled iron shell, and shell made from the much-extolled Gregorini iron from Italy. The gun used was an ordinary 9-inch Woolwich, with a charge of 65 pounds of powder, giving a striking velocity of 1500 feet per second. Every possible care was taken to obtain uniformity of strength and character in the plates fired at. These plates were made by Brown & Co., of Sheffield, were 12 inches thick, and of excellent quality throughout. Each of them was divided into pieces 4 feet square, and each competitor had a separate piece to fire each shell at. Each competitor fired two shells and the general result was that both the steel shells supplied by Sir Joseph Whitworth & Co, passed completely through the plate, and were left partially uninjured. All the others, especially those supplied by Herr Krupp and Herr Grusen wore broken to pieces by the impact, except the shells of the (French) Terre Noire Company which proved to be so soft that they bulged, and consequently retained so little penetrating power that the back of the plate was but little damaged. In every case, therefore, excepting that of the Whitworth steel, the projectiles were found to be valueless as shells for the purpose of penetrating armor and of retaining their bursting power after penetration.

QUICK STEAMING.—The famous torpedo boat Lightning, built by Messrs. Thorneycroft, has been beaten at last. Recently a trial was made by two launches constructed by Messrs. Yarrow & Co., of Poplar, for the Admiralty. The trials were carried out under the superintendence of Mr. Neil M'Dougall for the Admiralty. The boats are each 85 feet long, 11 feet beam, and draw 3 feet. They are strongly constructed of steel, and are fitted with compound surface-condensing engines capable of indicating 420-horse power. The high pressure steam cylinder of these engines is 12½ inches in diameter, and the low pressure 21½ in., both having a 12 inch stroke. These boats are at present known by their builders numbers, one being No. 419 and the other No. 420. The former is propelled by a three-bladed screw, 5 feet 6 inches in diameter and 5 feet pitch; and the latter by a two bladed screw of similar proportions. The trials were made over the measured two miles at Long Reach. No. 420 was first tried, and made the down run over the two-mile course in 5 minutes 19 seconds, which is equal to a speed of 22.59 knots per hour. In other terms, this vessel attained the remarkable speed of 26 miles an hour. She had six tons of ballast on board, and her draught forward was 2 feet 8½ inches, and aft, 2 feet 7 inches. Her mean revolutions were 460 per minute; *maximum*, 475; steam pressure 120 pounds; vacuum 23 inches to 25 inches and blast 4 inches. The tide had just turned and was running out, being, therefore, with the vessel on the run down. On the run up it was of course against her. This run was made in 6 minutes 47 seconds, or equal to a speed of 17.69 knots per hour. The mean of the two runs was 20.14 knots, or 23.2 miles per hour. On the up run the mean revolutions were 460 per minute; the steam pressure 120 pounds; the vacuum, 24 inches; and the blast 4 inches. The vessel was under way just an hour, during which time she burned 10 cwt of coal, a portion of which was used in getting up steam. No. 419 was then tried. She was run light without any ballast, her draught forward being 2 feet 5 inches, and aft 2 feet 4 inches. The first run was made up the river, and, consequently, against the tide. The two miles were run in 6 minutes 38 seconds, giving a speed of 18.09 knots per hour. The mean revolutions were 459, the steam pressure 110 pounds; the vacuum 22 inches, and the blast 4½ inches. The second run was made down the river, and, consequently with the tide. Here the two miles were accomplished in 5 minutes 1 second, giving a speed of 23.92 knots or more than a knot faster than any run made by the Lightning, or 27.56 miles per hour. The mean of the two runs was a speed of 21 knots, or 24.2 miles per hour. On the last run the mean revolutions were 459, the steam pressure, 110 pounds; the vacuum 22 inches, and the blast 4¾ inches, This is by far the highest velocity ever obtained by a boat or ship of any dimensions or under any conditions.

TORPEDO WARFARE.—A remarkable series of experiments has just been concluded at Cherbourg by the successful completion of the three hours' trial of the last of a set of six torpedo vessels, which Messrs. Thornycroft & Co. have just delivered to the French Government. These vessels are somewhat similar to the improved "Lightnings" which that firm is now building for the English Admiralty, being 87 feet long over all, by 10 feet 6 inch beam, and drawing about 5 feet 6 inches of water. They are made of thicker plating than the original Lightning, and differ from her also in having the rudder placed abaft the screw—an arrangement which it was feared would occasion a considerable loss of speed in the vessels, and which was only introduced at the urgent request of the French Government. By some what modifying the construction of the hull and introducing some improvements in the machinery, which practically secured an increase of available power, this fear, as will be seen from the following statement of results, has been completely dissipated, and the boats have in some cases attained a higher speed than the Lightning did on her trial. The results actually obtained were as follows:

No. of Boat.	Speed on Measured Knot. Knots.	Speed on three hours' Run. Knots.
54	18.482	18.661
55	19.423	18.734
56	18.441	18.963
57	18 379	18.165
58	19.152	18.405
59	19.307	18.836

The runs on the measured knot, six in number for each boat, were made alongside the breakwater at Cherbourg, and the three hours' runs were made in the open sea between Cape la Hogue on the one hand and Barfleur on the other. The difference of speed as ascertained are accounted for by the condition of the bot-

toms of the boats and the state of the wind and sea on the days of trial. The speed contracted for was 18 knots per hour, so the contractors have amply fulfilled their obligations in that matter. The consumption of coal at full speed was found to vary from 18 cwt. to one ton per hour, and the bunkers were capable of containing five tons of coal. The actual amount of coal carried on the trials was only that required for a three hours' run. Steaming easily, the consumption was found to be very light—one of the vessels, having made the voyage from Chiswick to Cherbourg in 22 hours on a consumption of 2½ tons of coal. The weight on board, in addition to the three tons of coal required for steaming, consisted of a crew of ten men, with stores, &c., including even a spare propeller and a weight equivalent to the weight of the torpedo gear to be used on the vessel, and fixed in the position that the gear will occupy when the vessel is on service.

The primary object of the French in having these particular boats is, of course, the defence of Cherbourg; but it does not require a great amount of foresight to perceive that boats which are capable of steaming from one end of the Channel to the other and still having coal for a two or three hours' run at full speed will not be confined to the defence of any particular port, but will, in conjunction with larger vessels, be employed in offensive operations which will leave little to be done in the way of actual defence. Engineers and stokers accustomed to other classes of engines and boilers find some difficulty at first in getting the power, and consequently the speed, which Messrs. Thornycroft & Co.'s men obtain; but this is mainly a matter of practice, and the French officers of the "Défense Mobile" are most assiduous in their efforts to acquire information regarding their new boats, and to practice their men in the working of them. Organization is principally what is now required to convert these boats, when properly armed, into a most important means of national defence; and the well-known ability of the French in this way may be safely trusted to supply that want, so far as they are concerned.—*Times.*

COMPOSITE ARMOR PLATES.—In continuation of the Admiralty experiments with armor plates, a composite plate, manufactured by Messrs. Cammell & Co. of the Cyclops Works, Sheffield, was subjected to gunnery tests on board the Nettle target ship, at Portsmouth Harbor. The experiments are to determine whether steel or composite plates, that is plates made with iron and steel, cannot be made of greater inpenetrability than the iron plates with which our war vessels are now coated. Already nearly a dozen plates have been in competition and notwithstanding each has represented from 300 pounds to 500 pounds the results obtained have not been altogether hopefull. The first experiments took place in the presence of a distinguished company including the Directors of Naval Ordnance and Naval Construction, and repre sentatives of the German, Italian and Russian navies. Since that occasion, however, the experiments have been conducted in private, being only attended by practical delegates of the Admiralty able to gauge results of the trials. The above-mentioned p ate was 8 feet long by 6 feet 8¾ inches in width, and 9 inches thick, its weight being slightly over eight tons. It was composed of 3½ inches of steel, and 5½ of iron. The plate was fixed to a transverse wood bulkhead built from side to side of the ship, and consisting of two vertical and two horizontal layers of oak bulks, making in all 3 feet 6 inches of thickness, the whole being shored by substantial wooden spalls secured by a massive thwartship. The gun used was a 12-ton 9-inch muzzle-loading rifle, and stood behind thwartship wooden bulkhead 30 feet from the plate. The charges were 50 pounds of battery pebble powder, and the projectiles chilled Palliser shots, 251 lbs. in weight, the muzzle velocity being 1420 feet per second, and the energy at the muzzle 3486 feet. Three rounds are usually fired at a plate and hitherto that number has done inevitable damage, but this plate was so comparatively invulnerable as to lead to two extra shots being fired to ascertain whether it was possible to break it up. The impact of the first three shots formed a triangular diagram, being about 2 feet apart. The first projectile struck the plate on the right hand side and penetrated nearly 7 inches, occasioning a series of superficial cracks. The impact of the next shot was on the lower section of the plate the penetration being a trifle more than 7 inches, and the further injury a fissure gradiating to the bottom of the plate, going quite home to the backing. The third shot made a number of cracks insignificant in their character, and penetrated 6½ inches. The depth of penetration needs to be explained for to those unacquainted with the previous experiments the idea may be conveyed that these tests were rather a failure. At ten yards distance, with so powerful a gun as a 12-ton 9-inch rifle, a shot penetrates clean through an iron plate, and partly through the backing, and in a lesser degree the same result has attended the experiments with composite plates, excepting in the case of that manufactured by Sir Joseph Whitworth, which was an extraordinarily expensive one, being studded with intensely hardened steel plugs. The fourth shot was aimed at the center of the triangular diagram, and partially broke the plate in two, the width of the fissure being ⅜ of an inch. Neither part, however, came away from the backing. The fifth projectile struck the right hand lower corner of the target and carried away the section bodily. All the five shots were smashed to fragments by the concussion, only their heads being imbedded in the plate. The experiments were conducted by Captain Herbert, of the gunnery ship Excellent. On Tuesday two more iron plates were received at Portsmouth Dockyard, one measuring 12 feet 8 inches by 4 feet 6 inches, its thickness being 10 inches, whilst the other's dimensions were 10 feet 5 inches by 4 feet 1½ inches, and its thickness only 2 inches. The former plate was manufactured by Messrs. Brown, of Sheffield, but the latter bears no maksr's name, although it is understood to have been forwarded by the same firm. Immediately after the receipt the

dockyard authorities telegraphed for instructions as to whether the plates were to be at once fixed into position for gunnery experiments.

BOOK NOTICES.

GEOGRAPHICAL SURVEYING: ITS METHODS, USES AND RESULTS. By FRANK DE YEAUX CARPENTER. New York: D. Van Nostrand. Price 50 cts.

This book is No. 37 of the Science Series. It is a report prepared originally as a part of the labor of a Commission for the Survey, Geological and Geographical, of the Empire of Brazil.

A complete discussion of the methods persued in the survey of large areas is presented in this little treatise.

The organization of the corps; the order of prosecution of different branches of the work; the comparative merits of different instruments, and the methods to be employed to secure the proper degree of completeness and accuracy without needless expenditure of time, are treated with a degree of fulness that leaves nothing to be desired by anyone familiar with the general methods of surveying.

The subject will interest many who are not of the engineering profession, since the results of the surveys of our great western plateau have called forth such flattering compliments from foreign scientific journals.

THE WHITWORTH PAPERS. I, Plane Metallic Surfaces; II, An Uniform System of Screw Threads; III, A Standard Diurnal Measure of Length. By JOSEPH WHITWORTH, Esq., Manchester. Price 20 cts. For sale by D. Van Nostrand.

These brief essays are all included in one small pamphlet, which seems singularly disproportioned to the importance of the topics or to the eminence of the author.

Practical engineers, however, for whom these papers are designed, generally regard brevity in books with favor, and will find these essays none the less acceptable because they are inexpensive.

RAILWAY SERVICE: TRAINS AND STATIONS. By MARSHALL M. KIRKMAN. New York: Railroad Gazette. Price $1.50.

This work treats of the composition and movement of railway trains and the laws governing the same, including an exposition of the duties of train and stationmen. The principal topics discussed are: The mysteries that underlie the organization and movement of trains; The different signals employed on different roads; Phraseology employed on English roads; Technical terms of a railway service; Classes and grades of trains and their movement; Instructions to conductors, brakemen, &c.; Rules regarding passenger and freight traffic; Austrian railways; English railways; General regulations for the block system on a double track road.

The work is well printed and will doubtless be of good service in aiding to harmonize different systems and improve in a general way the railroad management of the country.

PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS.—Excerpt Minutes.

The following papers have been received through the kindness of Mr. James Forrest, Secretary:

The Steam Navy, comprising papers on its use, by Chas. Douglas Fox, M.I.C.E.; James Brand, A.I.C.E.; Henry Mitchell Whitley, A.I.C.E.; Charles Augustus Harrison, M.I.C.E.; also Remarks on Steam Excavating Apparatus, by Ruston, Proctor & Co.

Machine Tools, by Percy Ruskin Allen.

The Egremont Ferry-Landing, by William Carson, M.I.C.E.

The Hooghly Floating Bridge, by Bradford Leslie, M.I.C.E.

Drainage and Cultivation of the Albufera (Marshes) in Majorca, by Henry Robert Waring, M.I.C.E.

All the above papers, except the last, are fully illustrated.

MISCELLANEOUS.

SOURCE OF ERROR IN LEVELING.—Mr. G. C. Herron, Ottawa, Can., writes us as follows: "I have not seen mention made of the fact, in any book on Engineering, that when leveling over a hill or mountain the bubble will not assume a truly horizontal position; but will be at right angles to a line from its center to the center of gravity of the general mass of the earth and hill combined. This will cause the line of sight to rise in going up a hill and to fall in going down, and is a fruitful source of error in correct leveling."

M. BARDOUX has opened at the Palais du Champ de Mars the Exhibition connected with Public Instruction. The minister said in his address that, owing to the recent progress of France, that country was now inferior to no other European nation as regards popular education. The results of the last conscription are highly satisfactory in this respect. Out of 294,382 men admitted into the ranks of the French army in 1877, only 4,992 were unable to read or write, 2,620 had taken their preliminary degrees in letters or sciences, 234,279 knew the "three R's," 36,325 could only read and write, and 5,856 could only read. Elementary schools have been established in the various regiments of the French army for years but the attendance, which had been very limited, is now almost universal. Not less than 305,989 soldiers were pupils of regimental schools in 1877; out of these, 255,380 followed the course of elementary instruction, 36,981 the secondary course, and 4,682 the course of superior instruction. The army has been turned into a machine for promoting elementary knowledge. In 1877 not less than 33,337 soldiers learned to read, 24,483 to write, and 111,303 were taught arithmetic. Under guidance of their officers, 200 soldiers from the garrisons of Paris visit the Exhibition daily.

"THE supply of ice in Bombay has failed," was the announcement which greeted

the inhabitants of that city and the surrounding country about the middle of last month; and no one who has not experienced a week of life in India without ice can conceive the dismay with which the report was received. A large trade in ice is carried on between India and North American ports, Boston being the principal place of shipment, and, with the special arrangements made on board the vessels for keeping down the temperature, it is found cheaper to import it in this way than to make it artificially. The man who can devise some means of making ice by artificial means, in large quantities and at a sufficiently low cost, will make his fortune and confer an immense boon on those whose fate it is to dwell in countries beneath the sun. A little enterprise would probably open up a new field for the supply of ice for India in the Antarctic regions The lands and seas surrounding the South Pole require exploration, and a vessel destined to press the icebergs of that region into the service of the inhabitants of India would be able to drive a lucrative trade, and at the same time do science a service. It would hardly be possible, perhaps, to take a giant iceberg in tow, and haul it bodily into Bombay Harbor, but with the easy means afforded by dynamite of breaking up these floating monsters into suitable sizes for stowing on board ship, the neglected supplies might, thinks the *Colonies and India*, be utilized with comparatively little difficulty.

LE NEVE FOSTER TESTIMONIAL FUND.—Some members of the Society of Arts, and others who know the history and progress of the society during the last quarter of a century, and feel how much of its success during that long term has been due to the judgment, zeal and devotion of its chief executive officer, the secretary, Mr. Peter Le Neve Foster, have associated themselves together to present him, on the occasion of his completing twenty-five years' service, with a substantial testimonial in money, as an expression of their respect. Mr. Foster became secretary to the Society of Arts in 1853; the number of members at that time was little over 1,000, and the annual revenue scarcely exceeded £3,000; whilst in the year 1877 the number of members was nearly 4,000, and the revenue over £11,000. A reference to its "Journal" will show how many are the important public questions with which the society has successfully dealt during this period, questions in the initiation and conduct of which Mr. Foster has taken a prominent part. Education, elementary and technical, the reform of the patent and copyright laws, international exhibitions, public health, Indian and Colonial topics—these are but a few of the subjects on which Mr. Foster, through his connection with the society, has done useful work. On grounds such as these his friends confidently appeal to the members and to the public for their hearty co-operation. A committee has been formed to receive subscriptions, which may be paid to the credit of the Le Neve Foster Testimonial Fund, at Messrs. Robarts, Lubbock & Co., or at Messrs. Cocks, Biddulph & Co., or to the honorary secretaries and treasurers, at the offices of the Society of Arts, John Street, Adelphi.

ONE of the most remarkable occurrences which has come under our observation lately is the disappearance of a locomotive and tender beneath the quicksands of Kiowa Creek, Colorado.

The circumstances are somewhat as follows: An eastern-bound freight train on the Kansas Pacific road, on the 21st of May, plunged at full speed into the above named creek, the bridge having been washed away by a flood. The current was so strong that loaded cars and iron parts of the locomotive were washed five miles down stream, while the locomotive and tender disappeared altogether and were not found for more than two weeks afterwards, though diligent and constant search was made with long iron rods and otherwise daily. They were finally discovered, it is reported, by means of a magnet, which was carried over the surface of the sand and was finally attracted by the hidden iron. They are fifteen feet below the sand and twenty-five feet down stream below the bridge. Specific gravity accounts for the sinking of the locomotive through the quicksands, but in our judgment the movement down stream can only be accounted for by supposing that the whole mass of sand in the bed of the stream was in motion, like a glacier, and that the combined weight of the sand and the force of the current were sufficient to force this ponderous mass of iron, weighing perhaps twenty-five tons, the distance of twenty-five feet from where it fell. It is calculated that water moving at a velocity of 3,600 feet an hour carries fine gravel, and when moving at a rate of two miles carries coarse gravel and pebbles. Such being the case, a stream moving with a velocity of not less than five miles an hour in a bed of quicksand would doubtless move the whole mass with almost irresistible force. It must be remembered that nearly all of the time, the year round, the bed of the Kiowa is perfectly dry and that all the water that flows through it except during freshets, passes beneath the surface of the sand, and it is not unreasonable to suppose that the sand may thus be moved *en masse* when suddenly saturated by a swift and powerful stream. Doubtless the formation of the canons of the plains may be, in part at least, accounted for in this way.—*Western Review.*

THE commission for reorganizing the Observatory of Paris has—says *Nature*—ended its sittings, as we have already reported. The commissioners recommended no change in the present organization of the Internal Meteorological Office; but, taking into consideration the actual wants of meteorology, it has advised the Minister of Public Instruction to appoint a meteorological commission, in order to suggest any measures which might be likely to promote the interests of meteorology at large, without interfering with the working of telegraphic weather forecasts sent by the International Office to the sea-ports and more than 1200 parishes all over France.